

1 October 1981

Is this the great crash of 1981?

No doubt it is simply coincidence that the fall of the stock markets in London, Hong Kong and New York last week should have come on the eve of the annual meeting in Washington of the International Monetary Fund and of the World Bank. The meetings were planned well over a year ago, while the stock markets could have slumped at any time. The coincidence is, however, timely, for some of the issues argued out this week in Washington also underlie the collapse of confidence in the financial communities of the world. And although the ways of the financial community are commonly supposed, by other professional people, to be either irrational or deliberately wayward, the behaviour of the financial markets on this occasion has a profound bearing both on the aspirations of the technical community and on its well-being in the future. Indeed, if the more gloomy suggestions in the more scary newspaper headlines are taken seriously, there is now a serious risk that beneficent technical developments of all kinds will be set back.

So what has gone wrong? The immediate answer, that people have lost confidence, is correct but not an explanation. People with money to invest have lost faith in the paper value of the securities offered for sale by governments throughout the world, which usually offer to return to the investor their face value in the currency concerned after a period of time ranging from a few years to a few decades. While individuals' loans to governments are outstanding, they are of course paid interest. They may not be unduly alarmed that their original investment will be eroded by inflation if the annual interest is sufficient. The persistence of high interest rates on government securities in both London and New York during the past few years is a measure both of the high rate of inflation and of widespread scepticism about the respective governments' management of financial affairs. (It is, of course, irrational that President Reagan, in the past few weeks, should have been complaining at the unwillingness of people to lend money to his Administration at less than five per cent above the inflation rate; who should prudently ask for less?) That gloom about the value of government securities has now spread to the market in industrial shares is a sign that even those who have chosen not to lend to governments are persuaded that government policies are not going to provide a climate in which industrial enterprises can prosper. In short, misjudged or not, the collapse in the stock markets reflects the opinion of a substantial number of knowledgeable people that the next few years will be a miserable time for industrial growth and development. The message is clear, and cannot be ignored.

But is the judgement correct? Unfortunately, the signs are that it is. In the United States, for example, President Reagan's second budget, announced in his television broadcast a week ago, is full of incidental proofs that the Administration cannot follow a coherent economic policy. Some parts of the economy are beyond its control and it is in any case committed to domestic and international policies that it cannot abandon for reasons that range from the political to the ideological. The objective, of course, is what every Republican dreams of — a balanced budget; like his predecessor, however, Mr Reagan can do no better than promise to attain this simple goal three years from now. In the meantime, according to the Administration's estimates, it will be necessary to borrow some \$70,000 million from the securities market. Both Congress and the financial community expect that the need will be much greater — and that, like President Carter's, Mr Reagan's final budget will be far from balanced. For the United States, the consequences will be deflationary if the supply

of credit is screwed down — theoretically, high interest rates should help in that direction, but nobody seems to know how high they should be — or inflationary, if the Administration decides that printing money is the easiest way out. As if, however, to tie its hands behind its back, the Administration is doing what it can to protect influential sections of the voting public from the economic facts of life, as with the steps that have been taken to keep mortgage rates within politically acceptable bounds. And Congress, with a mid-term election only a year or so away, has declined to budge on the President's proposal to reduce the growing charge on public funds represented by the way in which social security pensions are linked with the inflation rate. The President's refusal to reduce the military budget for next year by more than \$2,000 million has also been interpreted as a sign of willful disregard of the economic facts of life. The best hope now is that the slump on Wall Street will have given both parties pause. In the meantime, the decision (see p.328) that discretionary expenditure by the US government must be cut by 12 per cent across the board is a sobering reminder to the research community — conspicuous among those who will be affected — of the damage done by even the partial correction of financial misdemeanours.

The United States is not, however, alone in financial transgressions of this kind. Thus the pursuit of the monetary policy called monetarism in the United Kingdom in the past two years has been plagued by very similar mistakes. The full rigours of monetary restraint have been tempered, for special interests — in Britain, chiefly the nationalized industries — by policies designed to relieve discomfort and to avoid essential change. The result has been that industrial reform has been impeded and that the rate of inflation has remained much higher than would otherwise have been the case — last year, the prices charged by nationalized industries increased by 20 per cent, while those charged by the private sector increased by only 6 per cent. Appalling industrial relations have not helped. The consequence is that there are nearly three million people unemployed but no immediate prospect that it will be safe to lift the restraints of the past two years. The danger now is that this recession will be followed end-to-end by another triggered off by the collapse of the financial markets. Throughout this period, however, the British government has not faced up to the intellectual difficulty represented by the ambiguity of what is meant by money, the quantity of which it has assiduously but unsuccessfully sought to control, putting the health and welfare of the university system (and other longstanding institutions) at hazard in the process. Can it now hold firm to the policies of the past two years? Politically, but also economically, it is in a box. Changing economic course would be most simply accomplished by a different government. But a reflationary government would cause havoc in the present financial climate. Given that the United Kingdom is the most financially exposed of all industrialized countries, the next two years cannot fail to be a nightmare.

The British government will not, however, be the principal sufferer. If deficit financing by government is the cause of the collapse of the past few days, it is also the hallmark of those most at risk. And the principal exponents of this technique for getting by in the 1980s are the governments of the developing world, most of whom now owe money to the banks of the Eurodollar market (in turn financed by a mixture of OPEC surpluses and the export of inflation from the industrialized countries, the United States especially). Earlier this year, a consortium of European banks



agreed to accommodate the inability of the Polish government to pay its debts with a further loan. Last week, the same procedure was followed with San Salvador. One of the issues to be argued out this week is whether India should be permitted (as is its entitlement) to borrow more than \$7,000 million from the International Monetary Fund. The United States makes no secret of its view that the window should be closed, but has already been defeated on that proposition. The issue is, of course, quite simple. Should the impoverished developing countries of the world be subjected by their potential creditors to a financial discipline that appears to be as unpalatable in Washington as elsewhere, or should they be given the help they need in the secure knowledge that it will prove more burdensome than helpful? They, too, are in boxes of their own. Not borrowing means stagnation. Borrowing means a debt that can be serviced only by borrowing more. The second course is easier, because disaster is postponed. Yet disaster, when it comes, will mean more impoverishment, physical, intellectual and cultural. If the financial collapse of the past week continues, the beneficiaries of the Brandt Report will be the principal casualties.

So what is to be done? The only acceptable course is that the governments of the industrialized countries most affected by the stock market collapse should acknowledge that, however irrational it may be, it is also a fact of life. Somehow or other, they must cut their own expenditure still further until the financial markets are persuaded that there is prudence in public policies. It would be no shame if Mrs Margaret Thatcher, the British Prime Minister, or President Ronald Reagan, were to do this next week, even though both of them have been insisting that the courses they have been following are sensible in the circumstances. For the prizes at stake — no longer grand prospects of further progress in technology but merely the maintenance of what has been accomplished in the decades since the Second World War — are too important to be sacrificed to either ideology or pride. This time, however, each government should resolve that the consequences of prudent economic policy cannot be concealed from those affected. Better that pensioners (of whom there are a growing number) should be told that indexation of pensions cannot be afforded, or that employees of nationalized industries should know that their jobs are as much at risk as other people's, or even that the Third World should understand that advancement must be postponed, than that the whole fabric of the enterprise of prosperity created in the past thirty years should be thrown away.

Trade-off on missiles

The United States and the Soviet Union have at last fixed a date — 30 November — for resuming negotiations on arms control. What will they talk about and how urgently? Last week's communique after the meeting between the United States Secretary of State, Mr Alexander Haig, and the Soviet Foreign Minister, Mr Andrei Gromyko, was laconic to the point of obscurity. It referred merely to "those nuclear arms which were earlier discussed...". The reference is to the inconclusive meeting in Geneva just a year ago, when Soviet and American representatives apparently agreed that there was scope for negotiation about nuclear missiles based either in the Soviet Union or in Western Europe that could have strategic implications and quarrelled about the relevance of other delivery systems, aircraft in particular. Nothing was said last week to suggest that the quarrel has been resolved.

Even so, this agenda will go some way towards quietening the recent wave of public anxiety in Western Europe that the prospect of nuclear war has become more real than a mere bad dream. The vigour with which the negotiations are conducted will, however, also be crucial. If the Soviet and American negotiators announce, on 30 November, that their first step will be a substantial adjournment, public opinion will take fright — and Mr Helmut Schmidt will have to keep on worrying about the survival of his coalition government in West Germany. Yet such a delay is all too likely.

Although it has been plain since the beginning of the year that arms control negotiations would be forced on the superpowers, the Reagan Administration has pretended otherwise, with the result that its negotiating position is ill-prepared. The most serious danger, now, is that the United States will plump for some simple agreement with the Soviet Union without full consultation with the governments of Western Europe, which will in due course have to provide house-room for the Pershing II and cruise missiles. That could spell trouble.

The underlying difficulty is that the Soviet deployment of SS20 missiles has changed the strategic balance between East and West. Hitherto, it has been supposed that the strategic forces of the Soviet Union and the United States, capable as they each are of visiting unacceptable destruction on the other side, would be sufficient to deter a nuclear exchange. The Soviet missiles, which cannot travel far enough to reach the United States, are a threat to cities and military targets in Western Europe. The American plan to put missiles of similar range in Western Europe does not, however, fully redress the balance. To many Europeans, especially the hawks, the most frightening prospect is a nuclear attack on Western Europe in which the United States would be inhibited from using its new missiles for fear of a retaliatory attack on the continental United States. In other words, the proposed deployment of the American missiles is not a symmetrical counter to what the Soviet Union has put in hand. The doves in Europe, on the other hand, argue that the American missiles would make Western Europe hostage to pre-emptive nuclear attack, a view which overlooks the mobility of both the Soviet and American missiles but which has struck a resonant chord of public anxiety.

The simplest objective of the negotiations promised for November would be to limit the numbers of missiles of intermediate range deployed by the two sides. The standard assumption is that each SS20 carries three independent warheads, suggesting an agreement in which three American missiles are equated with one SS20. There are, however, several snags. First, the United States Congress and the new Administration have been talking loudly about the importance of means of verification in arms control agreements; even if the Administration now settles for flexibility, the Senate may ensure that the unavoidable difficulty of verifying an agreement on the deployment of mobile missiles makes ratification impossible. Second, there is force in the Soviet point about the relevance of other means of delivery than missiles. Aircraft in Western Europe (as well, no doubt, as in the East) are equipped with nuclear bombs ostensibly on the calculation that they might be needed in some "tactical" situations, but plainly the same aircraft can operate strategically. So the deployment of potentially strategic nuclear weapons in and near Europe cannot logically be separated from their other potential uses. That, however, is a path to a morass of interminable talk about the feasibility of counting nuclear bombs in the bellies of aircraft and the feasibility of nuclear-free zones in central Europe.

Such projects are well worth while, but should appear on some future agenda. The best course, for November, is that the United States should agree not to insist for the time being on the full rigours of verification (and that the Administration should explain why to the United States Congress) and that the Soviet Union should agree (again for the time being) not to insist that aircraft should be included. They should at the same time acknowledge that they must resuscitate the Salt 2 agreement, which needs to be reworked but also ratified, and which is logically inseparable from the talks on "theatre nuclear weapons" now promised. And they should also pledge themselves to multilateral negotiations on the military balance in Europe, taking full account of the deployment of tactical nuclear weapons in the process. Then they should seek to strike a balance between the Soviet force of SS20s and the planned force of American Pershing II and cruise missiles that will not simply confirm the intended *status quo*. A sensible first objective would be to reduce the planned deployment of potentially strategic missiles by a half. Nothing less will satisfy the European need.

US chemical weapons production plan

\$8,000 million

5-year spending

waits in wings

Washington

The US Defense Department is using allegations about the military use of supposedly Soviet-produced toxins in South East Asia to boost its campaign for increasing domestic spending on the production of chemical weapons, suspended by President Nixon in 1970 after widespread protest over the use of chemical agents by the United States in Vietnam.

The Reagan Administration is expected to announce a chemical warfare programme costing about \$8,000 million over the next five years. In addition to upgrading the defensive equipment available to United States armed forces, the money would be used to build a new arsenal of so-called binary chemical weapons.

A classified report, said to contain the principal arguments in favour of restarting chemical weapons production, was sent to Congress in January by President Carter as part of a budget request that included a projection by the Defense Department for spending about \$2,470 million on the development of such weapons over the next few years. However, a study by a panel of the Defense Science Board, and chaired by Dr John Deutch, professor of chemical engineering at the Massachusetts Institute of Technology and previously Under-Secretary in the Department of Energy, suggested that in view of reports of a growing Soviet capability, this projection should be increased by between three and four times. Mr Reagan is expected to follow this advice.

Congress has already demonstrated its enthusiasm for such a move. In May, the Senate approved a plan by the Reagan Administration to spend \$20 million on equipment for a binary nerve gas facility in Pine Bluff, Arkansas — the first step in the Carter-recommended programme.

The decision to proceed with the construction of the binary facility had been partly stimulated by unconfirmed reports that the Soviet Union had been using nerve gas in its occupation of Afghanistan. Similarly, there is still uncertainty about whether mycotoxins have been consciously used as weapons in the conflict between Vietnam and Cambodia — and if so, whether these were produced in the Soviet Union, and are the same agents that were used in Afghanistan, as some official statements have implied.

The present controversy emerged two weeks ago, when US Secretary of State Alexander Haig told a press conference in

West Berlin that the United States now had "firm evidence of the utilization of such weapons in South East Asia". This evidence turned out to be a single example of stem and leaf taken from a plant near the Cambodian border in an area said to have been sprayed with "yellow rain".

The State Department in Washington said that the sample contained "three potent mycotoxins of the trichothecene group" identified as nivalenol, deoxynivalenol and a third known as T-2. Although State Department officials admitted that the evidence was far from conclusive, by drawing attention to the similarities in the symptoms observed in those exposed to "yellow rain" and the known effects of the trichothecene toxins, a natural product of several species of the fungus *Fusarium*, as well as reporting that the levels of toxins were "up to 20 times greater than any recorded in any natural outbreak", the impression was left that their use had been deliberate.

Several United States scientists expressed concern last week at the haste with which scientific data from a single sample had been used to substantiate charges with wide implications. (Biological weapons are currently prohibited by international treaty.) They pointed out several holes in the State Department's reasoning; for example, where the State Department

claimed that such mycotoxins "do not occur naturally in South East Asia", Professor Matthew Meselson has pointed to published reports of T-2 being found on sorghum in India and in grain in Italy.

Privately, US officials admit that their data were made public earlier than they would have liked. Several other samples are currently being analysed; and the State Department was pushed into making an early public statement partly because some earlier results had been leaked to the press.

However, Reagan Administration officials have not been slow to use such slim scientific data as they have to maximum political advantage. Mr Haig's reference to the apparent evidence of "yellow rain" in his speech in Germany helped him to deflect some of the criticism of the United States for the decision to place nuclear weapons in Europe.

Secretary of Defense Mr Caspar Weinberger last week threatened — somewhat to the embarrassment of the Administration — virtually to reply to the Soviet Union in kind. Answering reporters' questions during an interview broadcast by the "Voice of America", Mr Weinberger said that the United States needed "to make sure that we have some kind of balancing forces" and that knowledge on the part of the Soviets that they might face "retaliation in kind might be a deterrent

UK universities to tackle new minister

The Committee of Vice-Chancellors and Principals of Britain's forty-four universities hopes to persuade Sir Keith Joseph, the new Secretary of State for Education, of the error of the government's decision to cut the grant to the universities by eight per cent over the next three years. It has called for a meeting with Sir Keith at the earliest opportunity.

Central to the committee's argument is that any savings the government hopes to make will be substantially eaten into by payments of compensation to redundant university staff. It hopes to persuade Sir Keith either to make the cuts over a longer period or to rethink the policy of charging economic fees to overseas students, which this year has resulted in a 35 per cent shortfall in the number of overseas applicants for university places. Although the number of overseas students taking up offers of places will not be known for a few weeks, universities estimate that the shortfall will be sufficient to add considerably to their financial difficulties.

The vice-chancellors, who met last week to discuss the cuts, also report that many well-qualified home applicants have not found university places this year. To meet the government's objectives, they also estimate that ten per cent fewer will be successful in 1982 and 1983. At a time when the number of 18 year olds in the popu-

lation is increasing, they argue that money would be better spent on providing educational opportunities than on paying off redundant staff.

Although some universities have begun to estimate the number and cost of redundancies, the position will probably remain uncertain until January, when the vice-chancellors plan to put a more detailed estimate of cost to the government. But Dr Albert Sloman, chairman of the vice-chancellors' committee, sees no reason to reduce his first estimate of 3,000 academic redundancies in the next three years.

The committee has also begun formulating national guidelines for redundancy payments. The objective is a scheme to ensure that all university staff are treated fairly. One snag is that contracts of tenure vary considerably between and even within institutions. Another is devising a scheme attractive enough to persuade staff to leave voluntarily and yet not so generous that the government would reject it out of hand.

The vice-chancellors have also embarked on rethinking academic career structures. They apparently consider that some form of tenure is essential, but plan to find a form of contract that also provides universities with a let-out in hard times. The aim is to eliminate "copper-bottomed" contracts with no let-out clauses, at present enjoyed by only some academics. **Judy Redfearn**

that would prevent these things from ever having been used”.

Following apparent complaints from State Department officials about the possible implications of the secretary's comments, the “Voice of America” added a statement to the interview reaffirming the United States's renunciation of biological and toxin weapons. It explained that “retaliation in kind” meant “redressing the imbalance through the development of an adequate protective posture”.

David Dickson

International nuclear agency

Changing sides?

Stockholm

The man who led the campaign in Sweden in 1980 to reduce nuclear power to only a stopgap option, to be abandoned when renewable energy sources become economic, has been appointed Secretary General of the International Atomic Energy Agency (IAEA).

The primary role of IAEA — a United Nations agency — has been to promote the peaceful uses of nuclear energy: a role which has given it a prominent part in the debate over energy supplies to the Third World and potential nuclear proliferation. So the appointment last week of Dr Hans M. Blix, 58, Sweden's deputy foreign minister and an environmentalist, is somewhat controversial.

Until 1978 Blix was a senior civil servant specializing in international law — in which he earlier held a professorship. But with the collapse of the 1976 conservative government he was drawn into politics on behalf of the Liberal Party. As deputy foreign minister he has concentrated on Sweden's aid to developing countries, making Sweden one of the few to reach the United Nations target of 1 per cent of gross national product. It is this interest and success with the Third World which has no doubt won him the secretary generalship of the IAEA.

In the 1980 referendum on nuclear power in Sweden, Blix campaigned for the winning “middle line”: increasing the number of nuclear power stations from 6 to 12 in the next few years, but then holding the number at 12 if alternative sources such as solar power or biomass (a realistic option in Sweden with its vast number of trees) are then viable.

However, Blix said this week in an interview with the Stockholm newspaper *Dagens Nyheter* that he did not see IAEA as a place to peddle his own views. His job will be to implement what can be agreed. He failed to mention that what can and cannot be agreed is, to a degree, under the influence of the IAEA secretary general.

There were three areas that IAEA should pursue, he said. First, its traditional role of furthering the peaceful uses of nuclear power — though he would like to see more nations adhering to the non-proliferation

treaty. Blix is greatly interested in the idea of the creation of a nuclear weapon-free zone in the Middle East. Although he has made no public comment yet on the question of the use and international control (through IAEA) of the French reactors supplied to Iraq and recently bombed by Israel for fear of Iraq developing nuclear weapons, he did say in the interview that the control of nuclear power was “very important” — and that that control works very well today.

The second area in which IAEA should give assistance is in nuclear safety, both in reactors and waste disposal, Blix said: here there should be an international exchange of experience and a degree of open debate. “I came to realize the importance of this during the Swedish referendum”, he commented. The third role for IAEA was support of developing countries in the

More science cuts

Washington

Further proposed cuts in American science programmes are expected to be announced shortly following President Reagan's announcement of an across-the-board cut of 12 per cent in the discretionary spending budgets of all federal agencies.

Basic science did relatively well in the President's original budget proposals to Congress in March, which envisaged keeping research spending virtually constant in real terms. The new round of cuts, made necessary by indications that even the original cuts were not likely to be sufficient to satisfy the Wall Street bond market, mean that spending on science in real terms would drop by at least 10 per cent in the fiscal year which begins on 1 October, the first drop since 1974.

Individual agencies, such as the National Science Foundation and the National Aeronautics and Space Administration (NASA), have yet to decide precisely where the cuts would fall. But the magnitude of the cuts — about \$600 million in the case of NASA — means that many research projects would inevitably be terminated.

In high energy physics, for example, Dr Sidney Drell, deputy director of the Stanford Linear Accelerator Center and chairman of the Department of Energy's high energy physics advisory panel, said on Monday that the result would probably mean about a 20 per cent reduction in the level of operations. He expected hundreds of personnel to be laid off from the country's national laboratories, and that many planned experiments would not now take place.

Addressing a meeting of the panel, the President's Science Advisor, Dr George A. Keyworth, said that the new rounds of cuts made his earlier remarks about the need to choose priorities for the support of science “even more relevant today than three months ago”. **David Dickson**

analytical and other uses of radionuclides.

Carl Tham, Swedish spokesman at IAEA, said this week that more important than Dr Blix's position during the Swedish referendum was his immense experience in international relations. **Robert Walgate**

Heidelberg cancer centre

Neurath's case

Dr Hans Neurath, whose resignation as Director of the German Cancer Research Centre at Heidelberg was announced in June (see *Nature* 20 August, p.665 and subsequent correspondence 24 September, p.252), gave the centre's board of trustees a critical and even bitter explanation on 23 September. In his statement, Dr Neurath said that he had found it impracticable to carry through agreed proposals for running the centre because of the circumstances, “structural, scientific and concerning personnel”, at the centre.

Dr Neurath's departure from the centre will take effect on 31 December. In his statement to the board of trustees, he noted that his appointment had been made so that he could improve the quality of cancer research at Heidelberg and “bring it up to an international standard”.

In reality, the statement continued, he had found the institution to be “plagued by serious and perhaps irreconcilable conflicts of interest”, largely stemming from an “old established majority”. Dr Neurath said that his impression was that “procedural questions and administrative and legal arguments” have priority over scientific work, and that scientific leadership is impeded by over-literal interpretations of the centre's charter.

Dr Neurath also said in his statement last week that he had been denied the active support both of the trustees to whom his statement was addressed and of the representatives of government ministries. He complained that those concerned had supported him by words and not by deeds, thus undermining the prospects for his success as director.

The statement added that Dr Neurath decided to resign in the hope that such a drastic step might contribute to reform.

Polish higher education

Bill progressing

Poland's controversial new bill on higher education is to go forward for discussion by the Council of Ministers “without essential changes”. So promised the Minister for Science, Higher Education and Technology, Dr Jerzy Nawrocki in a statement intended to put an end to “unjustified fears and extreme protest movements”.

The protests, which have now spread to virtually all universities and higher educational institutions in Poland, were triggered off by the the last-minute changes

which the ministry wanted to introduce into the bill, without the consent of the academic community. In particular, the academics said, the ministry had tried:

- To limit academic freedom.
- To restrict the autonomy of the universities in disposing of their financial resources.
- To extend the competence of the courts over students' organizations.
- To limit the right of workers in academic institutions to protest against discussions.
- To return to the former system of selection of university rectors, deans and collegiate bodies.

These amendments, the academics maintain, would virtually nullify the liberalization of academic life introduced in the past year which the bill was supposed merely to codify.

After a number of meetings with academics — and a stormy press confer-

ence with educational journalists on 15 September — the minister found himself facing a number of "strike alerts" from academics and students, due to take effect when the academic year opens on 1 October. Although the majority of protest messages stressed the academics' desire for peaceful negotiation, some hard-liners were less amenable. In particular, the Independent Students' Association at Poznan Technical University called for all negotiations with the ministry to be called off. The only bill to go forward to the Sejm (Parliament) for ratification, they said, should be that drawn up by the Codification Commission; Minister Nawrocki has now promised to put this bill before the Council of Ministers. However, he is adding an appendix giving the ministry's own views on disputed points. In particular, says Nawrocki:

- The minister should have the right to set an upper limit on university intake "so as to ensure a rational employment policy."
- The university, as a budgetary unit, cannot have the right "freely to shape its pay fund", but must shape employment within the framework of the pay fund allocated.
- The rector should have the right to call for the dissolution of an assembly on university premises if it infringes public order or strikes at the political and social system of the state.
- Academic staff should have six weeks vacation, not eight.
- Rectors should be elected either by the university senate or electoral college, or by the senate and college in conjunction, as envisaged by the university statute.

In fact, some of these points were never in dispute. The academics had sought only budgetary autonomy within the funds allotted, not for unlimited funding. Nobody had questioned that the rector might need to call for help to quieten a disorderly assembly; what was agreed in the Łódź accords which settled last winter's student unrest was that the police should not come onto a campus uninvited.

The idea of an upper limit on university intake is reasonable enough, since Poland now faces the grim possibility of up to 1 million unemployed if the economy is to be made to balance, and the Prime Minister, General Wojciech Jaruzelski, has committed his government not to pay unemployment relief.

If such a policy is to be implemented, however, the present atmosphere of academic autonomy would suggest that it should be agreed by the various rectors and senates concerned, acting through the new Permanent Conference of Polish Universities, not sprung on them suddenly.

This, in fact, remains the bone of contention — the fact that the ministry tried to introduce the changes unilaterally and with virtually no time left for academics to voice objections. In spite of the minister's statement, therefore, the tension continues.

Vera Rich

Research sponsorship

Agony at MIT

Faculty members of the Massachusetts Institute of Technology (MIT) are holding a special meeting today (Thursday) to discuss a controversial offer from Mr Edwin (Jack) Whitehead, the founder and former president of the laboratory instrument company Technicon Corporation, to fund a free-standing institute for research into molecular approaches to developmental biology.

Under the terms of the offer, \$20 million would be provided for the construction of a building in Cambridge to house the Whitehead Institute for Biomedical Research, which has already been formed, with Dr David Baltimore, at present American Cancer Society Professor of Microbiology at MIT, as its director.

Mr Whitehead will also provide an initial \$5 million a year to cover the running costs of the institute, enough to support 20 new research professors. MIT would receive an additional endowment of \$7.5 million to cover costs associated with its links to the institute, and eventually the Whitehead Institute would be provided with its own endowment of \$100 million to guarantee a permanent income.

Top administrators at MIT are enthusiastic about Mr Whitehead's proposals, which they see as maintaining MIT as one of the leading biotechnology research institutions on the East Coast.

Commission man

The European Commission has appointed an outsider as director-general of Research, Science and Education (DG XII) in succession to Dr G. Schuster, who resigned in August after a nine year stint. Paolo Fasella, professor of biochemistry at the University of Rome, takes over the post on 1 October.

Professor Fasella is, however, well versed in European research policy. He has been involved in the running of the European Molecular Biology Laboratory and in the years 1975–79 was president of the European Molecular Biology Conference. He has also served on various committees dealing with research policy in energy and biotechnology both at a European level and in his native Italy. Since 1973 he has been an adviser to the Italian government on such matters and has taken a particular interest in the problems of relating research results to industry.

One of Professor Fasella's first tasks in his new appointment will be to formulate a revised research strategy for the commission. In particular he will be devising the four-year indirect action programme which begins in 1983.

Professor Fasella's research work has been mainly in enzymology.

Judy Redfearn

Hidden spectre

Doubts have emerged about the validity of some important claims in photosynthesis research based on experiments carried out by Mr Mark Spector, much of whose recent work on protein kinases in relation to tumour viruses has been retracted by his erstwhile boss, Professor E. Racker of Cornell University (see *Nature* 10 September, p.93).

A paper published in the *Proceedings of the National Academy of Sciences* in February 1980 by Spector and C. Douglas Winget produced a considerable impact because it reported the purification from chloroplasts of the long sought-after enzyme responsible for the photosynthetic splitting of water to release oxygen. Other laboratories which hastened to confirm and extend this finding found difficulty in repeating the published experiments but, particularly when Spector ascended to the respectability of Racker's laboratory, these difficulties were put down to nothing more than the everyday problems of reproducibility of much biochemical work. Inevitably, however, there are now questions about the validity of the Spector and Winget claim as well as about an extension of the work reported by Dr Winget at the Fifth International Photosynthesis Congress held in Greece in September 1980.

Dr Winget, who did postdoctoral work with Professor Racker, is in the painful process of checking the experiments he published with Spector. Perhaps because he has not yet had time to check everything, the picture is confusing and not altogether comforting. Although he can repeat some parts, there are others which, despite considerable effort, he has been unable to reproduce. Dr Winget, reached by telephone on Monday afternoon, was afterwards unavailable.

Peter Newmark

However, the proposal has generated some controversy among faculty members over how compatible a virtually autonomous research institute would be with traditional academic practices. And the MIT debate is being watched closely by other research universities.

In the case of the Whitehead Institute, the debate has focused on the implications of the "dual allegiance" of the research staff. These will have joint appointments between the institute and MIT, and although the staff will be appointed and paid by the Whitehead Institute, MIT's own research and review procedures for recruitment would apply.

However, at a faculty meeting last month, several members asked whether academic staff paid for entirely by outside funds would participate fully in activities not directly related to their research, such as undergraduate teaching or participation on administrative committees.

Responding to some of these concerns, the Whitehead Institute has agreed that three MIT representatives should be appointed to its board of trustees — these are former president Jerome B. Wiesner, dean Abraham Siegel of the MIT Management School and Dr W. Gerald Austen, chief of surgical services at the Massachusetts General Hospital. Other trustees include Dr Donald Fredrickson, past director of the National Institutes of Health, and three members of the Whitehead family.

In a letter sent to faculty members before the September meeting, Dr Low said that

he and president Paul Gray had concluded that the benefits of the proposed arrangement for MIT seem to outweigh the potential problems.

At the faculty meeting itself, Dr Baltimore answered critics of the proposal by saying that he intended to make sure that staff members of the Whitehead Institute fully participated in activities apart from teaching.

In reply to a question about the decision by Duke University in North Carolina to turn down a similar offer from Mr Whitehead to fund an independent research institute seven years ago, Dr Low said that at the time Mr Whitehead's ideas had been more medically oriented. Having discussed the Duke decision with its president, Mr Terry Sanford, he said that he was convinced that there was nothing in the decision to deter a possible MIT affiliation.

The MIT Corporation, which is formally responsible for the affairs of MIT, will discuss Mr Whitehead's offer at its meeting tomorrow. Any conclusions reached or recommendations made at today's faculty meeting will be passed on to the corporation, although a spokesman for MIT said last week that he does not anticipate any substantial opposition.

It is intended that the Whitehead Institute should be independent of commercial interests. Although, like MIT, it will have patent rights to the results of its own research, no company will have preferential right to the licences. A final decision is expected at the corporation's meeting in December. **David Dickson**

European space industry

Landing platform

The European Parliament is urging the community of ten to formulate a long-term strategy for developing Europe's fledgling space industry. Last week, members of the parliament adopted a resolution calling for the speedy development of new space technologies that could enable Europe to compete with the United States space shuttle. In a vote that broadly endorsed the recommendations of a working document prepared by M. André Turcat, a French member of the parliament and an ex-Concorde test pilot, they also called for a conference of European space ministers at the earliest opportunity.

The parliament's initiative is slightly unusual. Space policy has been largely left to those nations that have their own programmes and the members of the European Space Agency (ESA), not all of whom belong to the European Community. The initiative, however, comes at a time when the space agency is battling with the task of resolving national differences in drawing up a medium-term plan. The politicians are anxious that Europe should make a greater and more united effort.

M. Turcat argues in his document that the effort should be directed to competing in the growing world market for applications satellites and the vehicles to launch and even recover them. He estimates that the European space industry will grow to at least 10,000 million European units of account (1 EUA = £0.59) in the next ten years. Influenced by the recent decision of the National Aeronautics and Space Administration to cancel its contribution to the International Solar-Polar Mission, he also argues for greater European independence.

That could be achieved, the document claims, if Europe were to develop technologies for establishing space stations and building unmanned, reusable vehicles capable of servicing them. Such a view will hearten members of the French national space agency, CNES, which is shortly to submit to ESA a proposal for a partly reusable launch vehicle. But enthusiasm from other quarters may be muted. Not all members of ESA are keen to devote the agency's limited budget to such a costly project.

The differences, however, may be resolved if, as the document argues, more money is devoted to space. That will be for the individual governments to decide. But one possibility, suggested in the debate on the document, is that the European Investment Bank should step in.

Such a major project would also involve some reorganization within the European space industry and possibly extending the role of ESA. While acknowledging that co-operation between Europe's diverse space industries has come a long way in the past six years, the document calls for even greater cohesion, suggesting collaboration

UK industry fights genetic guidelines

British industry is unlikely to be satisfied by the relaxation of guidelines for the industrial application of genetic manipulation which the Genetic Manipulation Advisory Group (GMAG) plans to approve this week. The new guidelines have been through several drafts since they were first discussed earlier this year. The latest draft removes the present obligation on industry to give warning when it plans to use cultures of more than 10 litres when laboratory-scale experiments with the microorganisms are exempt from notification. Instead, companies would be required to notify the group only at the start of their operations. But notification of large-scale operations with organisms classified for laboratory purposes in category 1 or above would still have to be given well in advance.

The spur for revising the guidelines came earlier in the year, when the Confederation of British Industry (CBI) produced its own recommendations. In particular, it urged that processes using microorganisms exempt from regulation at the laboratory level should be exempt from notification, and that notification alone should be sufficient for large-scale operations with category 1 micro-

organisms. In all cases, the confederation argued, the same basis should be used for the assessment of large-scale and laboratory manipulation, but GMAG does not agree.

CBI's proposals echo its longstanding discontent with GMAG as such. Thus the confederation has urged that processes using microorganisms in higher categories of containment should be controlled by the newly reconstituted Advisory Committee on Dangerous Pathogens, on the grounds that too many regulatory bodies are involved, but also on the grounds of confidentiality. It also suggests that GMAG should transfer its responsibilities to the Health and Safety Executive, which should recruit its own staff inspectors and, when necessary, take advice from the Dangerous Pathogens Committee.

CBI's recommendations would effectively rob GMAG of control of industrial processes. GMAG's members, however, believe that the group should at least be aware of all new activities. The likely outcome is that GMAG's draft guidelines will be approved but that the dissenting voice of CBI will ensure that the debate continues. **Judy Redfearn**

along the lines of the successful aeronautics conglomerate, Airbus-Industrie. Government money should be available, not only for development within ESA but also to encourage industry.

The more immediate and less contentious goals, supported by the parliament, include an early decision to go ahead with Ariane 4 — a modification of the current Ariane, whose fourth test flight is now scheduled for 14 December, that will launch heavier payloads — and the formulation by the European Commission of a feasibility study for a large space station.

If nothing else, last week's vote could result in a meeting of space ministers with the opportunity for thrashing out a realistic policy.

Judy Redfearn

US-Soviet exchanges

Students to stop

Washington

The US Congress, in a little-noticed manoeuvre, is trying to cut off all federal funds used to support Soviet students studying science and technology subjects in US universities under the official exchange programme between the two countries. The proposed ban would remain until restrictions are removed which prevent American students from learning about such subjects in the Soviet Union.

The move, which would be made through an amendment to the budget of the State Department and is said to have the department's support, would affect about 40 Soviet citizens who take university courses in the United States every year. It is part of a campaign by certain members of Congress to restrict Soviet access to American technology and expertise.

The amendment would prevent both the State Department and the International Communications Agency after 1 July 1982 from financing any long-term scientific or technological study in the United States by citizens of the Soviet Union in the US-USSR Graduate Student/Young Faculty Exchange, or in the USSR Senior Scholar Exchange.

Such exchanges are organized through the New York-based International Research and Exchanges Board (IREX), which this year has placed 31 Soviet scientists at US institutions including Massachusetts Institute of Technology, Stanford University and the University of California, Los Angeles.

Some officials responsible for the educational exchange programme have for some time complained that US students who visit the Soviet Union under the same programme are restricted to studying the arts, humanities and social sciences, and are not permitted to look at aspects of Soviet technology.

Introducing an amendment to the State Department authorization bill on the floor of the House of Representatives last week,

Mr Paul Findley, a Republican Representative from Illinois, described it as an attempt to ensure "the mutuality of any academic exchanges that may occur between the Soviet Union and the United States in the future".

The amendment was passed unanimously. And although the bill in its present form has been temporarily shelved to await further budget decisions by the Administration, a spokesman for Mr Findley said last week that the Senate had already indicated its support for the amendment, which he said was "almost certain" to become law when the State Department's budget was finally agreed.

Although scientific exchanges with the Soviet Union were substantially reduced by the Carter Administration in response to the occupation of Afghanistan, they are still continuing. At the National Academy of Sciences, for example, visits by US scientists to the Soviet Union arranged on an exchange basis with funds from the National Science Foundation have dropped from 100 to 60 a month.

The proposed congressional action would therefore only affect a part of the academic exchange programme between the two countries, although others may well fall victim to the budget cuts now under discussion in Congress.

Mr Alan Kassos, director of IREX, said last week that if the amendment passed into law it could cripple access by American students to the Soviet Union. IREX disputes Mr Findley's charges that visiting Soviet scientists are being given free access to US technology which could be of military or commercial value. The 31 currently studying in the US, for example, were all intensively screened by federal agencies such as the State Department, the Department of Defense and the Department of Commerce — a process which excluded a further 23 potential students on the grounds that their presence could violate export controls on scientific and technological data.

Replying to the charge that there is an imbalance in the subjects studied by US and USSR students, IREX points out that each side selects candidates whose proposed field of study is felt to meet national interests. "The US side chooses to send candidates in the humanities and social sciences because we feel it is imperative to know about the dynamics of Soviet society" said Mr Dan Matuszewski of IREX, adding that IREX had also occasionally organized visits by research workers in fields such as mathematics and plasma physics.

Officially the State Department still favours such academic exchanges as one window that was kept open when other bilateral exchange agreements were cut back after the occupation of Afghanistan. IREX still hopes that it will be possible to head off the Findley amendment when it comes up for debate again on the floor of the House.

David Dickson

Attack on refusniks

The Soviet authorities appear to be launching a concerted campaign to stop the various Jewish refusnik seminars from reconvening in this academic year. Especially heavy pressure has been applied to the oldest — the Moscow Sunday Seminar, founded in 1973, which, since the arrest of Viktor Brailovskii in November 1980, has been hosted by his wife Irina. Ten leading members of the group — including Dr Brailovskaya herself, and also Dr Aleksandr Ioffe and Dr Yakov Al'pert, have been threatened with official reprisals, ranging from expulsion from Moscow to imprisonment if the seminars continued, and all have been placed under overt police surveillance.

In the case of the Khar'kov engineering seminar, matters have proceeded further than threats. Its leader, Aleksandr Paritskii, recently deprived of his degree of Candidate of Science, was arrested early in September and now faces charges of slandering the Soviet Union. (This is, apparently, in connection with a letter which he and some forty other refusniks signed more than a year ago, asking the Communist Party to intervene with the passport authorities, so that they could leave for Israel.)

For Dr Aleksandr Lerner, however, the approach was somewhat different. Dr Lerner, who filed his application for emigration in 1971, has on occasion been informed that he would "never" be allowed to leave. Last month, however, he was given to understand that if he discontinued his cybernetics seminars, a visa might be forthcoming. Dr Lerner's case has attracted considerable attention abroad — indeed, a special "Lerner Circle" of fellow cyberneticists has been established — and this may account for the difference in attitude. In July, when Dr Lerner's wife died, the Soviet authorities granted an unprecedented visa to their daughter, Dr Sonya Lerner-Levine, now working at the Weizmann Institute in Israel, to return to Moscow for the funeral.

Support from foreign colleagues is also believed to have influenced the authorities in the case of Viktor Brailovskii who is now serving a sentence of five years' internal exile. Dr Brailovskii was convicted on 18 June, but was not transferred to his place of banishment until September. This was not, as had been feared, Siberia, but the village of Beineu in Kazakhstan, which has a far milder climate. And instead of being transferred from prison in a closed railway wagon, he was flown to Beineu. These were concessions made by the legal authorities on account of his weak state of health but Irina Brailovskaya believes they would not have been granted without international pressure.

Vera Rich

CORRESPONDENCE

Tanks and tactics

SIR — Allied military command will doubtless hope that enemy tank designers take John Baruch's advice (*Nature* 27 August, p.792) and add an extra 5 tons of shielding to each 30-ton tank. This would increase the cost of transport of enemy tanks by $(5/30) \times 100 = 17$ per cent. Meaning either they would have 17 per cent less acceleration (so would be easier to hit with conventional rockets) or, if their acceleration performance were retained, would require 17 per cent more fuel and have drive and weight-bearing parts 17 per cent stronger, further adding to their weight.

Though never even primed, the neutron bomb would serve its purpose by making enemy tanks ungainly and more vulnerable to conventional weapons.

OLIVER MILBURN

Leeds, Yorkshire, UK

SIR — I was very disturbed by your leading article (*Nature* 13 August, p.571) favouring the neutron bomb, designed to be an anti-tank weapon, killing tank crews within a 1,500-foot radius while minimizing property damage. The countermeasure is simple: the Russians need only attack in loose formations maintaining a distance greater than 1,500 feet between tanks.

Tanks are best suited to warfare in open country where there is little property to destroy. Tanks used in built-up areas are vulnerable to weapons much less sophisticated than neutron bombs — such as Molotov cocktails, as the Russians themselves proved, to the Nazis' chagrin, in the battle of Stalingrad. And anyway such built-up areas are likely to contain large numbers of Western European civilians who are no less vulnerable to high-intensity neutron radiation than are Russian tank crews.

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Toxic edible oils

SIR — We read with interest the account by Hilton and Gillman in the *Sunday Times* (23 August 1981) describing the tragic outcome of the use in Spain of industrial oil in place of edible olive oil. The industrial oil was based on low erucic acid rape seed oil plus grape and cotton seed oils, all of which contain high levels of unsaturated fatty acids.

There are many diseases in farm animals which can be regarded as being induced directly by the degradation of normal dietary intakes of unsaturated fatty acids. This is especially so if the natural antioxidants vitamin E and selenium are present in insufficient quantities, in tissue, to interrupt the process of peroxidative damage. The tissues most usually affected are muscle, heart, liver and brain.

Nutritional degenerative myopathy (NMD) can occur when young ruminants deficient in vitamin E and selenium are turned out to young grass which is normally high in unsaturated fatty acids¹. When such animals consume this material the unsaturated fatty acids largely escape conversion to saturated fatty acids (hydrogenation) in the rumen and are absorbed². That this is the source of unsaturated fatty acids for lipid peroxidation has been recently confirmed experimentally³.

Animals with a normal status of vitamin E and selenium are not affected, showing that these micronutrients are essential in protecting against peroxidative damage.

What makes these studies particularly relevant is first that disease is induced by feeding unsaturated fatty acid. Second, many of the symptoms seen in calves affected with the peroxidative disease (NMD) are very similar to those described in the people affected in Spain — symptoms of respiratory distress, muscle weakness and pain, along with an inability to rise. Changes also occurred to teeth and skin. Deaths have also been attributed to consumption of the oil.

The oil used in Spain, as well as being rich in unsaturated fatty acids, had been heat treated to 200°C to make it acceptable. Heat treatment of unsaturated fatty acids can induce peroxidative damage with the formation of toxic hydroperoxides, and destroy vitamin E. Indeed Spanish physicians found vitamin E to be the most appropriate treatment for the disease.

Both the recent Spanish experience and our own experiments highlight the necessity of ensuring that unsaturated fatty acids are used correctly. Molecular damage resulting from peroxidative pathways can result in alteration in lipids (membranes), proteins (enzyme activity) and nucleic acids⁴. This gives considerable potential for both short and long term pathogenic processes. Thus where the consumption of unsaturated fatty acids occurs and especially where their use is actively encouraged, care must be exercised to ensure that tissue peroxidation is not an extremely unpleasant sequel. Vitamin E and selenium dietary intake levels must be adequate to ensure that this is not the case.

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Those specimens

SIR — For readers of *Nature* of 3 September (p.6) Brian Ford has readily blamed staff of this journal for a particular sentence (and the title?) of his article (*Nature* 30 July, p.407) reporting on "Leeuwenhoek's specimens discovered after 307 years". But his tightly literal answer is hardly impressive when it is compared with his concurrent article, and allied response regarding earlier knowledge of Leeuwenhoek's specimens, in *New Scientist* (30 July, p.301; 3 September, p.619).

I am particularly surprised when he dismisses Clifford Dobell's fine standard book on Leeuwenhoek (1932 and 1960) and Professor Cole's work of the 1930s as merely that of "occasional individuals half a century ago". Oddly, he now says the statement about the existence of Leeuwenhoek's specimens made on p.333 of Dobell's book is referred to in his own main article in *Notes and Records of the Royal Society*. This will be a welcome variation on the available accounts in *Nature* and *New Scientist*, but it is difficult to adjust

existing criticism at the time of writing this letter because publication of the *Notes and Records* cited by him has been delayed until at least the end of September.

Mr Ford is right to be enthusiastic over his examination of Leeuwenhoek's original specimens with a scanning electron microscope (ideally suitable for unfixed dried biological material), but he only has himself to blame in turning attention away from this main theme.

R. DEREK WOOD

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Balance of powers

SIR — I do understand why Dr Jeffery (*Nature* 27 August, p.791) should have used the retail price index to correct for inflation in the calculation of relative energy costs, except insofar as it mirrors the wholesale price index. One hardly buys power stations retail. Neither do I know of any investment giving a steady return of 5 per cent above inflation rate.

At the same time I accept that there is an uncertainty about the effective total cost of either coal or nuclear energy which may well be comparable with the difference between the figures given by Dr Jeffery and those given by the Central Electricity Generating Board (CEGB). Neither party would get much credit in an examination for quoting figures to better than 1 per cent without any estimate of probable error.

But the importance of the controversy lies in its relevance to future action. Power stations started in the next few years will be in the middle of their working lives in about 25 years' time, and "guesstimates" as to the cost ratio at that time are so uncertain that the differences between Dr Jeffery's figures and those of the CEGB are quite negligible. Nearly all the likely changes favour building more nuclear power stations. The cost of oil is likely to be so high that it will be more expensive for transport than synthetic liquid fuel so more coal may be needed to produce this than is at present used for all purposes. Concurrently, electric transport at least for buses and heavy lorries may increase the total demand for electricity, requiring more electric power than at present, even though current "guesstimates" neglecting this possibility suggest that we shall need less.

We ought to be offering the Third World cheap coal now, instead of lecturing them on the undesirability of burning down their forests for fuel, but surely by the year 2005 the Third World demand for mass-produced consumer goods will be relatively less and its demand for energy more, so that our most valuable exports could be coal, wind-generators and solar electric plants — if we have any to spare.

Finally, if the charges on capital should increase by a further large proportion, which increases the relative cost of nuclear electricity, this will discriminate still more heavily against the solar, tidal, wind and wave alternatives, almost the whole of the costs of these being charges on capital, so that nuclear power may still be the cheapest way of making up whatever cannot be supplied by coal. As of now it is the only means capable of the scale of output needed.

JOHN FREMLIN

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NEWS AND VIEWS

Gene families, hopeful monsters and the selfish genetics of DNA

from Miranda Robertson

WHEN molecular biologists first acquired the technology for cloning DNA, people predicted almost every possible new development from panacea to pandemic; but no-one foresaw that it could open the door on a startling and unexpected view of the evolutionary history of the eukaryotic genome. Existing mysteries, such as the extremely high content of noncoding repetitive DNA in many genomes, have become no less mysterious: indeed, far from helping to resolve the question of whether these sequences have a regulatory function, recent discoveries about the rate and pattern of their evolution have made them in some ways more mysterious. At the same time, it has become clear that both coding and noncoding DNA sequences are capable of surprisingly rapid and wide-ranging migration in the genome.

These revelations have confronted molecular biologists with some extraordinarily difficult questions that have traditionally been the concern of evolutionary geneticists. Phrased in modern molecular terms, they are: what are the phenotypic effects of noncoding DNA and the lability of the genome, and how do they bear on the control of development and the origin of species? The issues raised by these questions are similar to those explored, in more general terms, by the geneticist Richard Goldschmidt, a passage from whose later writings was taken as the theme of a recent meeting* at which molecular biologists and geneticists met to discuss them; and where Goldschmidt's hopeful monster (see ref. 1) was reborn as the product of the transposition of small regulatory elements of DNA, or the translocation of large chunks of genome, leading in either case to major changes in gene expression by means of which, according to a flight of fantasy indulged by W.F. Doolittle (Dalhousie University), a toad might evolve into a princess with a minimum of intervening millennia.

Repetitive DNA and speciation

Do the recent molecular data in fact lend weight to Goldschmidt's notion of the origin of species in "single macroevolutionary steps", or provide evidence for the changes in genomic rearrangements that could give rise to them? The first question was addressed by R. Flavell (Plant Breeding

Institute, Cambridge) and G. Dover (Cambridge University) who asked whether the rapid evolution of large families of repetitive DNA could act as a blunt instrument driving speciation by reproductive isolation alone, without reference to adaptation.

Flavell quoted as an example the *Gramineae* species, 80 per cent of whose DNA consists of families of middle repetitive sequences, interspersed sometimes with one another and sometimes with unique DNA. Both the sequence and the distribution of many repetitive families vary considerably between closely related species². Clearly, by creating difficulties with homologous chromosome pairing, these differences may contribute to the infertility of interspecies hybrids, and thus help maintain the reproductive barrier between existing species. But whether they could have arisen within a species and led to speciation through reproductive isolation is an open question. Furthermore, H. Rees (University College of Wales, Aberystwyth) pointed out that quantitative differences in DNA between species may not always result in failure of chromosome pairing in hybrids, as illustrated in *Festuca*. On the other hand, Flavell and his colleagues find that developmental problems and reduced fitness in wheat-rye hybrids (triticale) are correlated with the amount of highly repetitive DNA on the ends of the rye chromosomes^{3,4}; although the repetitive DNA has no known effect on the phenotype of the adult plant.

The mouse species discussed in the same light by Dover raise even more complicated questions. He has used DNA hybridization and restriction analysis to identify families of dispersed repetitive DNA that apparently have similar copy numbers and distributions but different, though clearly related, sequences in four species of rodent⁵. This pattern of conservation of copy number and distribution but variation in sequence between species is known as concerted evolution (see ref. 6). The problem is how to explain the homogeneity of these repeats within species given the substantial variation between species. The easiest explanation, in the case of tandem arrays, is unequal crossing-over between sister chromatids or homologous chromosomes. This would lead to the continual expansion and contraction of the repeated sequences, the eventual upshot of which can be shown statistically to be sequence homogenization within a population.

Unequal crossing-over is not, however, a satisfactory explanation for the concerted evolution of dispersed sequences; to explain their homogeneity Dover postulates that gene conversion takes place. In brief, this involves the formation of a heteroduplex between strands of homologous DNA at any two sites in the genome, and results in the 'correction' of regions of non-homology between them. In this way, sequence homogeneity could be maintained between widely separated homologous DNA sequences without any change in their copy number or distribution^{7,8}. While this could account for the relative homogeneity of dispersed repetitive families within species, it is more difficult to explain how the sequences originally became dispersed. Dover's controversial suggestion is that whatever unknown mechanism accounts for dispersal could also drive a variant sequence to fixation in a population and thus result in 'accidental speciation'. The crux of this proposition is Dover's substitution of the notion of drive, by which a mutant sequence has a high probability of superceding the wild-type sequence, for that of drift or natural selection, in which the probability of a mutant takeover is extremely low since both unequal crossing-over and gene conversion are inherently as likely to convert mutant sequences back to wild type as to convert wild-type to mutant sequences. Dover's point is that the horizontal spread of variant sequences between chromosomes would lead to non-mendelian segregation, which would increase the probability of their taking over from the wild type: this is what he calls drive.

Selfishness and dispersal

Horizontal dispersal of DNA sequences is well established in the case of bacterial insertion elements, the transposons of *Drosophila*⁹ and the retroviruses of birds and mammals, the pattern of whose boundary sequences marks them out as mobile elements. But there is a growing number of sequence families the molecular basis for whose dispersal is entirely cryptic. The repetitive rodent sequences discussed by Dover are one example; actin genes are another; and more recently, Childs *et al.*¹⁰ have reported dispersed histone and rRNA pseudogenes that seem to have escaped

*A discussion meeting on Genome Evolution and Phenotypic Variation was held in Cambridge on June 22-24, 1981. The proceedings, edited by G.A. Dover and R.B. Flavell, will be published by Academic Press.

Miranda Robertson is an associate editor of Nature.

from the tandem arrays of sea urchins and *Drosophila*, and Leder *et al.*¹¹ have discovered two dispersed α -globin pseudogenes in mice.

It is possible that the vector for these wandering genes is a retrovirus; but — to return to the problem of the concerted evolution of dispersed sequences — such a random vector could account little better than unequal crossing-over or gene conversion for the replacement of a wild-type by a mutant sequence. Thus we are led to the question of whether a mutant sequence could have some inherent advantage over the wild-type one, lending these otherwise neutral events a high probability of working in its favour. And this brings us to the issue of selfish DNA, which was the stuff of Doolittle's monstrous princess and a recurrent theme of the conference. The defining feature of selfish DNA, as agreed by Dover and Doolittle¹², is that its sequence should, by an as yet unknown mechanism, enhance its own propagation. This distinguishes it from ignorant DNA, which is also propagated but whose sequence is presumed not to contribute to its propagation. What selfish DNA and ignorant DNA have in common is that they have not evolved as a result of any functional effect on the organism. But it does not follow, as has been repeatedly pointed out, that such DNA will necessarily have no evolutionary consequences; 'accidental speciation', for example, might be a consequence of selfish or of ignorant DNA but could not be its function; and the existence of pseudogenes, which are clearly functionless, might increase the probability of future adaptive change and have profound effects on evolution.

Regulation and dispersion

By no means everyone accepts that noncoding DNA may be either selfish or ignorant; in particular, E.H. Davidson (California Institute of Technology) argues that the ignorance is all ours, and both the sequence and the site of noncoding DNA could have profound significance for the regulation of gene expression. If this is the case, then there should be evidence of evolutionary constraints both on the sequence of noncoding DNA and on the mobility of gene families with related functions demanding coordinated regulation. Such evidence was reported by A. Jeffreys (Leicester University) for the mammalian β -globin gene family, which consists of five functional genes and two known pseudogenes, all clustered on the same chromosome and separated by noncoding DNA some of which is repetitive. Since the β -globin genes are thought to have originated by duplication from the ancestral gene that also gave rise to the α -globin genes, which are on a different chromosome, the obvious question is whether the β -globin family has remained clustered through several rounds of duplication simply by historical accident, or because of the need to coordinate the

activities of the different members of the family during ontogeny. The recent analysis by Jeffreys and his associates of the intervening DNA in the cluster in three primate species shows that it is very highly conserved, with a rate of divergence of only 0.2 base pairs per million years¹³. This strongly suggests that the intervening DNA, which constitutes more than 90 per cent of the total β -globin cluster, may have important functions in regulating the genes.

A history of duplication and dispersion can equally be read in the distribution of actin genes, which are expressed in the cytoskeletal proteins of all cells, and, with only minor sequence variations, in the contractile proteins of smooth and voluntary muscle cells. Davidson argues from his studies of the actin genes of sea urchins that any minor differences in sequence of the highly conserved coding portions of the genes are almost certainly irrelevant to their different functions, which he believes to be determined by surrounding regulatory DNA sequences. By way of preliminary evidence he described possible determinants of the differential expression of the actin genes during early development. Sea urchins have at least 11 actin genes that can be assigned to five types on the basis of differences in the pattern of their intervening sequences and the sequences of their flanking regions. At least some of these genes are linked, but the linkage groups do not parallel homology in either the pattern of the intervening sequences or the flanking regions. From a comparison of oocyte with early embryonic gene expression, it seems that regulation of expression of different members of this family is correlated, not with their linkage groups, but with sequence of the 3' non-translated flanking regions¹⁴.

Further to his thesis that the evolution of a gene may depend as much on the genomic company it keeps as on the evolution of its sequence, Davidson quoted some recent protein sequence data of Vanderkerkhoeve, who finds that the muscle actin of scallops is homologous to the cytoskeletal, rather than the muscle, actin of vertebrates: a possible case in which the

differential expression of a gene rather than its sequence may have determined its function.

Transposition and phenotype

Speculations of this sort are at present beyond the skill of science to test but it is, however, worth distinguishing between local and global regulatory effects. An example of local regulation would be the activation of a cellular gene through the insertion of the proviral DNA of a retrovirus next to it¹⁵. Local activation is presumably operating in the case of the sea-urchin actin genes described by Davidson. Global regulatory phenomena may operate through control of the relative condensation of extensive domains of DNA, and thus govern the accessibility of large areas of the genome to activating proteins. The effects of such global regulation can, however, sometimes be seen on a single gene, for example in the position effect mutations of *Drosophila*, in which large-scale chromosomal inversions can inactivate a particular gene by transposing it to the edge of a region of condensed heterochromatin.

It is possible that the repetitive nature of some noncoding DNA could contribute to such global control mechanisms. For example, G. Roizes (Montpellier University), discussing bovine heterochromatin, pointed out that a prerequisite for DNA methylation, which is enjoying considerable popularity as a possible control mechanism, is a repetitive GpC sequence.

The only case in which the movements of genes and the existence of repetitive noncoding sequences have a clear effect on phenotype and obvious adaptive consequences is that of the immunoglobulin genes, whose antics were reported by H. Zachau (Munich University) and T. Rabbitts (MRC, Cambridge). Genetic recombination in somatic cells of the lymphopoietic lineage makes possible the expression of a much larger repertoire of antibodies than is encoded in the germ line, and furthermore enables each N-terminal antigen-binding site to be expressed with a range of different C-terminal effector sites. Specific repeated sequences are implicated in these somatic recombination events¹⁶.

But otherwise, effects on phenotype were barely mentioned at the conference — an omission deplored by J. Maynard Smith (Sussex University) on the grounds that natural selection acting on the phenotype is the principal force in evolution. One difficulty is that phenotypic effects of genomic upheavals may be elusive: Maynard Smith quoted his own experience with *D. pseudoobscura*, in which large chromosomal inversions are extremely common and have no apparent effects on morphology at all. However, as a professional evolutionary geneticist he agreed to try to assimilate the non-mendelian behaviour of the eukaryotic genome but he urged molecular biologists to note that "people have been thinking about evolution for a long time, and some of their conclusions may be worthy of attention". □

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Ancient astronomers of the New and Old Worlds

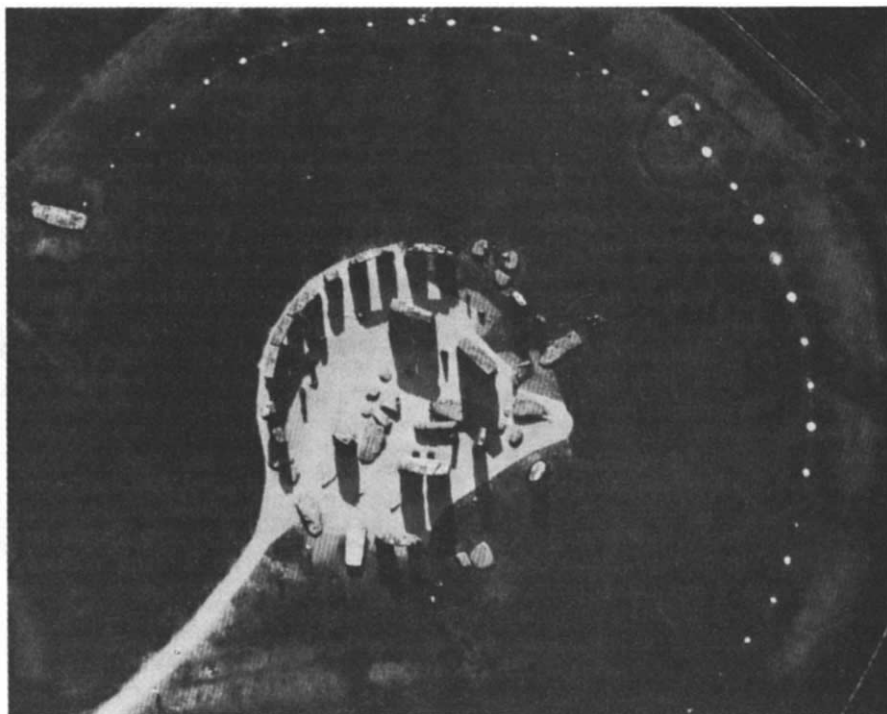
from Aubrey Burl

STONEHENGE has its astronomical counterparts and some, as the participants at a recent symposium* were reminded, are in the Americas. The symposium, held at Queens College, Oxford, took as its major themes the trends and progress in archaeo-astronomical studies in the Americas and Europe.

A.F. Aveni (Colegate University, New York) began by describing the six sources of information on the astronomical practices of early American societies: codices, stelae, ceramic inscriptions, Hispanic literature, modern ethnology and the architecture of surviving structures. These riches were far beyond the impoverished relics European prehistorians have to work with but our envy was modified when other speakers, including G. Brotherton (Essex University) and J. Remington (University of New Mexico), revealed the problems of reconciling Mesoamerican Long Counts with the astronomical calendars. To understand that the number 2,914 might have been devised to integrate the Venusian, solar and lunar cycles was, perhaps, not grossly difficult but why the Maya should have computed a reckoning of 3,669,565 years most Europeans must have found incomprehensible.

Conversely, when speakers discussed ancient American societies and their use of celestial events the participants were at one, for these were questions that confronted all of us, numerate and innumerate alike. F.G. Lounsbury (Yale University) offered us a splendid piece of detection, showing how a glyph at the little-known Maya site of Bonampak revealed that its people had calculated a favourable date for a raid on a neighbouring kingdom. The augural and ceremonial rather than 'scientific' use of astronomy was a favoured theme. Whether discussing the ceque system of the Incas (R. Zuidema, University of Illinois) or the symbolism of the Hopi Indians (S. McCluskey, West Virginia University), a people who paradoxically laid out precise alignments without the aid of astronomer-priests, there were few suggestions of 'science for science's sake'. Instead, it seemed that the Sun, moon, Venus and the Pleiades were objects observed for practical purposes, to establish a calendar, to determine the time for planting and to indicate propitious days for ritual.

These interpretations were repeated when the fragmentary evidence for early



Aerial view of Stonehenge. From *Rites of the Gods* A. Burl (Dent, 1981).

European astronomy was discussed.

The orientation of Early Copper Age graves in Hungary (K. Barlai, Konkoly Observatory, Budapest) and of North European Neolithic burials (W. Schlosser, Astronomisches Institut der Ruhr-Universität) supported the belief that in prehistoric Europe people had regarded the Sun and moon, in part, as symbols of death. Accordingly they laid out graves, albeit rather casually, so that corpses might face the Sun or moon. This is an astronomical interpretation quite different from that advocated in recent years by Alexander Thom and his co-workers. From their researches they have concluded that megalithic alignments were often very precise, sometimes designed to permit observers to discern such minuscule celestial events as the moon's 'wobble' at its standstills. A. Thom did discuss this, demonstrating the accuracy of 42 lunar lines, but it was a view disputed by C. Ruggles (Cardiff University) whose own field data offered no support for such precision.

Throughout the symposium there was an insistence that more rigorous criteria should be accepted by anyone engaging in archaeo-astronomical studies. The writer of this report asked that sites should be examined not in isolation but in culturally related groups to avoid the mischance of analysing an unrepresentative monument. Others (E.W. MacKie of the Hunterian Museum, Glasgow, and P. Freeman, Leicester University) requested that a

balanced consideration of astronomy and geometry should be adopted and proposed that an austere use of statistical material would prove advantageous.

It was with this healthy controversy still alive that on Tuesday morning the participants were guided around some megalithic sites in Wiltshire by R. Atkinson of Cardiff University. The current policy (if that is an appropriate word) of the Department of the Environment restricts scientific visits to Stonehenge to between 8am and 9am on Tuesdays in summer and meant that a 6 am departure from Oxford was necessary. After the privilege of entering the sarsen circle we visited other monuments — Woodhenge, Avebury, West Kennet and Silbury Hill.

It was a good conference. There was a marvellous site at Fajada Butte, New Mexico, where, high on the sacred slopes, Amerindians had propped three slabs to show the times of the solar solstice and the lunar extremes. These finely positioned stones cast shadows onto two carved spirals almost identical with those known in the British Isles.

Understanding Fajada Butte may help European researchers to understand their own silent megaliths better and it is to be hoped that another symposium can be arranged in the near future . . . and certainly not a Maya Long Count away! □

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*An archaeo-astronomical symposium, sponsored by the International Astronomical Union and the International Union for the History and Philosophy of Science, was held at Queen's College, Oxford, from September 4 to 9. It was organized by M.W. Hoskin of Churchill College, Cambridge, and limited to seventy invited participants.

Extragalactic jets: facts and fancies

from Peter Scheuer

PHILIP MORRISON (MIT) called them "fountains of radio stuff" but for most participants at the IAU symposium* on extragalactic radio sources they were "jets". Narrow bands of radio emission linking compact nuclei and extended structures have now been observed in nearly 100 sources, and their interpretation as high-velocity streams of plasma has become widely accepted. But there the certainty stops: we do not know whether high velocity means 3×10^5 or 3×10^8 m s⁻¹ or something in between. Of the twenty questions posed by Jan Oort (University of Leiden) at the outset, the only ones to attract a substantial number of attempted solutions were the question of whether jets are relativistic and the related question of whether jets in powerful radio sources are one sided intrinsically or only appear so because of relativistic beaming.

After reviews of extended radio structures and the hot gas around them revealed by X rays, Bridle (National Radio Astronomy Observatory, NRAO) summarized the systematic features observed in a large sample of jets with the Very Large Array (VLA). Feeble radio galaxies generally have two jets, emerging from opposite sides of the nucleus, with the magnetic field sometimes oriented along the jet close to the nucleus, but predominantly transverse along most of their length. Powerful radio galaxies exhibit jets more rarely, on one side only, and with the magnetic field preferentially parallel to the jet. This magnetic field geometry correlates even more strikingly with the radio power of the nucleus than with the total radio power. Variations of surface brightness and of width along the jets provide clues to their interaction with their surroundings. They do not fade with distance at anything like the rate a free ballistic jet should, nor do they expand sideways with a uniform or monotone increasing angle. These two facts indicate that the jets are confined by some external pressure and that they suffer some dissipation of energy. There is even a hint of longitudinal magnetic field along the borders of the transverse-field part of the jet in NGC6251, suggesting shear. The need for external confinement is particularly severe for the powerful jets discovered in several quasars by Wardle, Potash (Brandeis University) and Dreher (NRAO); the pressure required exceeds the ambient pressure indicated by X-ray observations and lacks a convincing explanation.

Depolarization of jet NGC6251 (Willis, Westerbork University) by Faraday

rotation gives an estimate of its density, and leads (via an energy argument given by Saunders *et al.*) to an upper limit for the jet speed of a few tenths of *c*. This is a crucial result, but the observations are not yet unequivocal, and the depolarization could still arise from rotation in irregular structure in front of the jet. Indeed, slow (a few 100 km s⁻¹) emission line regions have been observed at the edges of radio structures in some sources (Heckmann, University of Arizona; van Breugel, Kitt Peak National Observatory; Fosbury, Royal Greenwich Observatory; Price, University of New Mexico) and the radio emission is often depolarized where the emission lines are seen.

Wilkinson (Jodrell Bank) showed some of the beautiful maps now being made with the multiple telescope radio link interferometer, some with a dynamic range of thousands. Conway (Jodrell Bank) used such a map of 3C273 to argue that its structure is intrinsically one sided. Smarr (representing Norman; Winkler, Max Planck Institute, Munich; Smith and himself, University of Illinois) reported numerical simulations of the axisymmetric fluid flow caused by a jet propagating into a still gas. Their numerical code uses several clever techniques and a much finer grid than earlier work, and should become a powerful theoretical tool.

Readhead and Pearson (Caltech) reviewed the morphology revealed by about 200 'proper VLBI maps' (those made with closure phase and adequate *u-v* coverage) of some 50 sources at various epochs and/or frequencies. The majority have some well resolved structure, and most of these are of the familiar highly asymmetric core + jet type. In six sources that also have large-scale symmetric double structures, the jet is well aligned with it; where the large-scale structure is one sided the alignment is less good, and sometimes it lies on the side of the nucleus opposite the VLBI jet. Perley (NRAO) reported a weak asymmetric extended structure in about half of a large sample of 'compact' radio sources.

There are some VLBI pseudo-doubles, which look fairly symmetric at one frequency but reveal at a higher frequency that one of the components is a dominant core which was heavily self-absorbed, but there are others that appear to be genuine symmetric VLBI doubles with similar steep spectra and some tens of marc s separation. So far there is no evidence for (or against) superluminal motion in these 'genuine doubles'. Cohen (Caltech) reviewed superluminal motions; of seven sources properly mapped at several epochs, six show superluminal motion (3C120, 179, 273, 279, 345, NRAO140) ranging from 2.8*c* to 8.2*c* or

even 10*c*, while 4C39.25, with similar radio power and redshift, stands still (*v* < 0.2*c*) (all speeds given in this report correspond to *H* = 100 km s⁻¹ Mpc⁻¹). 3C179 (Porcas, Max Planck Institute) is of particular interest as it was discovered by its large-scale double structure, which dominates at low frequencies, so there is no obvious possibility of any selection in favour of a jet aligned close to the line of sight. Mutel (University of Iowa) reported that BL Lac had a large outburst and turned itself into a triple with apparent motions of 4*c* and (if the weak new component was ejected rather than just becoming brighter) 20*c* relative to the middle component. Romney (Max Planck Institute) showed that 3C84 (NGC1275), which had earlier resembled a pumpkin, had also turned itself into a plausible triple, but with an expansion rate of only 0.27*c*.

There is a complex of overlapping but non-identical types of source, called BL Lac (characterized by weak or no emission lines), OVV (optically violently variable) and HPQ (high polarization quasars); the latter were discussed by Moore, Stockman and Angel (University of Arizona). Most quasars are < 2 per cent polarized in the optical continuum; the few having stronger polarization (typically 4–20 per cent) are mostly OVVs with steep optical spectra, and all are radio sources (except PHL5200), but with no extended radio lobes. Most superluminals (but not all: 3C273 and 120 are exceptions) are HPQs. The 'relativistic jet' interpretation of these would be that they are extreme examples of jets seen end-on, so that the optical radiation, too, is dominated by polarized synchrotron continuum from the jet, the emission lines are therefore relatively weak, and the extended radio emission observed in about half of them appears in projection like a roundish halo (Stannard, Jodrell Bank) instead of a jet or a double structure. However, they are clearly not archetypal quasars seen exactly end-on, for their apparent luminosities would then be far too high, and besides, the BL Lacs originate in the nuclei of giant ellipticals (Weistrop, NASA-Goddard Space Flight Center), like radio galaxies of moderate power, whereas quasars are associated with ellipticals of normal stellar luminosity (Wehinger; Wyckoff, Arizona State University; and Gehren, Max Planck Institute).

Low-frequency variability is now firmly established and was reviewed by Fanti (University of Bologna). It sets a par-

*The IAU symposium no.97 was held at Albuquerque, New Mexico on 3–7 August 1981.

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ticularly testing problem for the relativistic beaming theories popular among VLBI observers, as the high apparent brightness temperatures (up to 10^{15} K) require high- γ jets very close to the line of sight, and consequently cause statistical embarrassments in hiding the numerous jets not pointing at us. It was therefore particularly interesting to hear of two classes — those whose variations correlate well with variations above 1 GHz, and the rest whose low-frequency variations may have a different physical cause — and that BL Lac is the archetype of the former class. Unfortunately very few objects can as yet be classified in this way.

Sherwood and colleagues (Max Planck Institute) reported a very high detection rate of optically selected quasars at $\lambda \sim 1$ mm; the fluxes lie on an extrapolation of the optical spectra and well above the extrapolated radio spectra. Infra-red observations were reviewed by Rieke (University of Arizona) and exploited by Lilly (Royal Observatory, Edinburgh) and Puschell, Owen and Laing (NRAO) to obtain spectrophotometric redshift estimates for very distant radio galaxies.

The fact that radio-quiet quasars have stronger Fe II lines than radio-loud quasars now appears well established (Oosterbrock, University of California, Santa Cruz; B Wills, University of Texas), but the weakness of the iron lines may be associated only with the steep radio spectrum sources with extended structure while the flat-spectrum core-dominated sources have iron lines more like the radio-quiet quasars.

The observations of large-scale (≥ 1 kpc) jets strongly suggest that their speeds are a few tenths of c or less, while the prevalence of fast variations and superluminal velocities (always outward) makes it difficult for VLBI observers to abandon the idea of ultrarelativistic jets on a small scale. Apart from statistical difficulties in slowing down jets without quietly losing very large amounts of energy. The only case where we are sure of the speed is the galactic source SS433, in which VLA observations incontrovertibly show a pair of precessing jets (thanks to a period of 164 days instead of megayears). Hjellming (NRAO) presented a comprehensive analysis leading to a distance of 5.5 kpc and an ejection speed of $0.26c$, the latter confirming earlier optical work. How far should one take the analogy with larger specimens? Only one or two of the 200 or so maps shown resembled SS433 (specifically, one shown by Gower, University of Victoria), though several more showed possible evidence of precession. If a jet originates near a black hole it is expected to be fast, but not with a lorentzian factor ≥ 5 unless the jet consists of electrons and positrons rather than electron-ion plasma (Rees, University of Cambridge). Thorne (Caltech), in a magnificent exposition of the ways of precessing a black hole and of

Exploiting geomagnetic data

from S.R.C. Malin and D.R. Barraclough

It costs about £20,000 per year to run a modern, efficient geomagnetic observatory. (It costs very little less to run an obsolete, inefficient observatory, but that is another story.) Whilst annual mean values are not the only output from observatories, they are some of the most valuable, particularly for studies of the main geomagnetic field and its secular variation, which provide the only information we have on the detailed dynamics of the Earth's core. Accurate estimates of the secular variation require a continuous series of annual mean values extending over a number of years. In view of the high cost of acquiring these data, it is desirable that the effort put into analysing them does full justice to their information content. This has seldom been possible in the past.

Over the past few years, the development of inverse theory (see the review by Parker *A. Rev. Earth planet. Sci.* 5; 35, 1977) has provided the means of deriving not only optimum models from given data sets but also reliable estimates of their uncertainty. Because of the physical complexity of the Earth's magnetic field and the mathematical complications of inverse theory, the application of this method to geomagnetic field modelling has been long delayed. In a recent paper, Whaler and Gubbins (*Geophys. J. R. astr. Soc.* 65; 645, 1981) have gone some way towards rectifying this.

Most previous attempts to model the geomagnetic field have used the classical methods of spherical harmonic analysis and have paid scant attention to errors. Whaler and Gubbins examine the validity of such procedures in the light of the recent developments in inverse theory and enquire whether a better description of the field might not be obtained from a somewhat different analysis of the data. Much of the work is involved with the mathematics of casting the problem in a form suitable for treatment by linear inverse theory. As an example of the use of the method, the authors apply it to a set of data based on observatory annual mean values and compare the resulting spherical harmonic coefficients with

those derived by the classical method. Such a data set is very small compared with those used in modelling the main geomagnetic field but is similar to those used in some studies of the secular variation. The authors concede that computational costs would prevent the extension of the method to large data sets. Although inverse theory can, in principle, give realistic estimates of the uncertainties of the resulting model, several technical problems have yet to be solved before such estimates can be obtained in the geomagnetic case. The main conclusion of this part of the paper, that the coefficients derived by inverse theory have little advantage over those derived by classical means, may seem trivial. However, this is far from being an obvious result and it is valuable to have it set on a firm mathematical basis.

The derivation of spherical harmonic coefficients is only part of the problem. For studies of core dynamics we require optimum estimates of the values of the field components at the surface of the core, together with their uncertainties. Whaler and Gubbins examine this part of the problem in terms of the spherical harmonic coefficients and the distribution of the data from which they are derived, again using the methods of inverse theory. While they reach no very firm conclusions, their formulation of the problem and their investigation of various smoothing procedures break valuable new ground and lay the foundations for future work.

Application of inverse theory to the modelling of the main geomagnetic field and its secular variation should thus help to exploit more fully the costly observations which are the basis of our knowledge of the field. Whaler and Gubbins have made a beginning; it remains for field modellers in general to assimilate their results and apply them, where possible, to the production of future models.

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extracting energy from it, showed that if one feeds it a balanced diet, including magnetic field, it will inevitably produce a Poynting flux and very probably electron-positron pairs.

Baldwin (University of Cambridge) discussed the evolutionary tracks of individual sources in the size-luminosity plane. On the evolution of the source population and the double quasar, Maarten Schmidt, in a conciliatory mood, allowed that as many as 2 per cent of quasars could be local objects, while Barnothy appeared at last in his rightful

role as a member of the astronomical establishment.

Finally, in the most intriguing talk of the symposium, Rudnick (University of Minnesota) exhibited transparencies to demonstrate that hotspots and other bright features are not truly symmetric, but rather interleave when their positions are reflected through the optical nuclei. Rudnick concluded that the nuclear engine ejects on one side at a time; but there are stranger implications than that, which will shed much new darkness on theoretical models if the facts are confirmed. □

Plasma diagnostics in an astrophysical setting

from David J. Helfand

PLASMA DIAGNOSTICS using high-resolution X-ray spectroscopy is a field of expanding importance in both fusion reactor research and high-energy astrophysics. In the laboratory, the goal is to measure temperatures and densities of the confined plasma and to chart the spatial and temporal distribution of impurities, which, through radiation losses, limit the all-important density/confinement time product in Tokamak machines. The aims of the astrophysicist are remarkably similar: to determine the temperature and density distribution in the emitting region and to derive the elemental abundances of the hot gas. The primary difference between the two fields is the factor of $\sim 10^{20}$ in the distance of the observer from the source.

The Einstein Observatory X-ray telescope was launched nearly 3 yr ago, carrying instruments designed to probe celestial sources emitting radiation in the 0.1–5 keV band (corresponding to temperatures of 10^6 – 10^8 K). Although many of the most spectacular early results were obtained with the observatory's two imaging detectors (see J.L. Culhane *Nature* **284**, 509, 1980 and D.J. Helfand *Nature* **285**, 133, 1980), nearly 20 per cent of the satellite's lifetime was devoted to spectroscopy. The increasing number of significant new results beginning to emerge from these observations serve to emphasize the coming of age of X-ray astronomy as a result of the Einstein mission.

Supernovae are the dominant generators of hot gas in the Galaxy. The expanding shock from the explosion heats the ambient

medium to temperatures of 10^7 – 10^8 K, ionizing the diffuse gas, evaporating cold interstellar clouds and enriching the material with heavy elements generated during the lifetime of the exploding star and/or in the explosion itself. For ~ 10 yr the expansion is adiabatic, with the kinetic energy of the shock being steadily converted to the thermal energy of the hot gas. Then radiation losses become significant and the temperature of the supernova remnant (SNR) begins to decline, until ultimately, the ions recombine, the shock dissipates and the remnant becomes indistinguishable from the interstellar medium.

This general model of SNR evolution has been discussed for over a decade, but many important aspects, such as the dynamics of the shock wave in a non-uniform medium and the amount of heavy element enrichment, are just beginning to be explored both theoretically and observationally. Early data from the moderate-resolution instrument on Einstein, the solid-state spectrometer built by the Goddard Space Flight Center, showed strong lines of silicon, sulphur, argon and magnesium in the spectra of several young SNR (Becker *et al. Astrophys J.* **243**, L73; **235**, L5; **237**, L77; 1980), implying that the X rays arose in a multi-temperature medium containing substantial overabundances of the silicon group elements. The instrumental resolution of ~ 150 eV was, however, insufficient to resolve the individual tran-

sitions which can be used as sensitive probes of plasma conditions within the sources.

The *Astrophysical Journal Letters* (15 May) reports the first results on the X-ray emission from SNR obtained with the other major spectrometer on Einstein, the focal plane crystal spectrometer (FPCS) built by MIT. The instrument consists of a curved crystal placed at the focus of the X-ray telescope; X rays meeting the Bragg condition are reflected from the crystal onto an off-axis, position-sensitive proportional counter. The crystal can be rocked back and forth during observation to scan a small band of energies (typically ~ 50 eV) around a preselected spectral feature. The instrument covers the energy range 0.2–3 keV with a resolving power $\Delta E/E$ of 50–500. Due to the small effective area of the device (~ 1 cm²), long integrations (up to half a day) are required to scan each potential line.

The data presented are for the relatively old SNR Puppis A; nearly continuous coverage over the range 500–1,100 eV (representing 4 days of observing time) has been achieved and a total of eight isolated lines and three line blends detected. The dominant features are the resonance lines of helium-like oxygen and neon ions, although other ionic species of these elements and lines of iron and nitrogen are also detected. All the features have been previously observed from the hot coronal plasma above active regions on the Sun.

Several factors determine the observed line intensities, including the temperature distribution of the plasma, its ionization

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100 years ago

THE FOG

A remarkable phenomenon occurred in New England on September 6, almost exactly similar to one that occurred in the same region on May 19, 1780. The *Springfield Daily Republican* describes it as follows: In this city the day began with a slow gathering of fog from all the watercourses in the early hours, the thin clouds that covered the sky at midnight seemed to crowd together and descend upon the earth, and by sunrise the atmosphere was dense with vapour, which limited vision to very short distances, and made those distances illusory; and as the sun rose invisibly behind, the vapours became a thick, brassy canopy, through which a strange yellow light pervaded the air and produced the most peculiar effects on the surface of the earth. This colour and darkness lasted until about three o'clock in the afternoon, once in a while lightening, and then again deepening, so that during a large part of the time nothing could be done conveniently indoors without artificial light. The unusual complexion of the air wearied and pained the eyes. The grass assumed a singular bluish brightness, as if

every blade were tipped with light. Yellow blossoms turned pale and gray; a row of sunflowers looked ghastly; orange nasturtiums lightened; pink roses flamed; lilac-hued phlox grew pink; and blue flowers were transformed into red. Luxuriant morning-glories that have been blossoming in deep blue during the season now were dressed in splendid magenta; rich blue clematis donned an equally rich maroon; fringed gentians were crimson in the fields. There was a singular luminousness on every fence and roof-ridge, and the trees seemed to be ready to fly into fire. The light was mysteriously devoid of refraction. One sitting with his back to a window could not read the newspaper if his shadow fell upon it — he was obliged to turn the paper aside to the light. Gas was lighted all over the city, and it burned with a sparkling pallor, like the electric light. The electric lights themselves burned blue, and were perfectly useless, giving a more unearthly look to everything around. The darkness was not at all like that of night, nor were animals affected by it to any remarkable extent. The birds kept still, it is true, the pigeons roosting on ridge-poles instead of flying about, but generally the chickens were abroad. A singular uncertainty of distance

prevailed, and commonly the distances seemed shorter than in reality. When in the afternoon the sun began to be visible through the strange mists, it was like a pink ball amidst yellow cushions — just the colour of one of those mysterious balls of rouge which we see at the drug-stores, and which no woman ever buys. It was not till between five and six o'clock that the sun had sufficiently dissipated the mists to resume its usual clear gold, and the earth returned to its everyday aspect; the grass resigning its unnatural brilliancy and the purple daisies no longer fainting into pink. The temperature throughout the day was very closed and oppressive, and the physical effect was one of heaviness and depression. What was observed here was the experience of all New England, so far as heard from, of Albany and New York city, and also in Central and Northern New York. In reference to this phenomenon the *New York Nation* suggests that it may be worth the while of weather-observers to note the approximate coincidence between the interval separating the two dark days in New England (May 19, 1780, and September 6, 1881) and nine times the sun-spot cycle of eleven years.

From *Nature* **24**, 540, October 6, 1881.

structure and elemental abundances, energy-dependent absorption by interstellar material along the line of sight to the source and the sometimes rather poorly known atomic physics parameters. By taking appropriate ratios of line strengths, however, many of these uncertainties can be eliminated. For example, comparison of lines from different ions of the same element (such as O VII and O VIII, Fe XVII and Fe XVIII) leads to a well defined ionization temperature for the plasma, assuming ionization equilibrium obtains. This assumption may be tested by comparing the relative strengths of the singlet and triplet lines from a single ionic species; for Puppis, although some net ionization of He-like to H-like ions may be occurring, the plasma seems close to equilibrium, as would be expected for a remnant of this age ($\sim 5,000$ yr). The required temperature range of $2-5 \times 10^6$ K probably exists as a result of density inhomogeneities in the initial structure of the interstellar medium before it was heated by the shock.

Likewise, taking ratios of O and Ne to Fe line strengths and then comparing these with the same ratios in the solar corona allows elemental abundances to be derived which are substantially independent of many of the uncertainties. Choosing lines of nearly equal energies for two elements eliminates the effect of interstellar absorption and implies nearly equal Boltzmann factors in the expressions for line emissivities; comparing these ratios with solar ratios then eliminates any remaining errors in the adopted atomic physics parameters. The analysis of the FPCS spectrum of Puppis indicates a clear departure of the O/Fe and Ne/Fe ratios from normal cosmic values. Although the data do not distinguish between an underabundance of iron, due, for example, to trapping in interstellar dust grains, and an overabundance of oxygen and neon, the former is difficult to reconcile with current ideas about grain formation and destruction. An overabundance of oxygen would be expected if ejecta from the inner regions of the exploding star were mixed with normal-abundance interstellar matter. However, as the remnant has already swept up >100 solar masses ($M_{\odot}$) of the interstellar medium, the absolute amount of new oxygen required is rather high: $2-18 M_{\odot}$ depending on the distance to the source and the exact value of the mean oxygen overabundance. This would require that the supernova progenitor was a very massive star ($> 25 M_{\odot}$), marking Puppis A as the remnant of one of those relatively rare events which may dominate the chemical evolution of the Galaxy.

The Puppis spectrum is among the first of the FPCS SNR data to be published. Exciting new results soon to follow include the discovery of an asymmetry of several thousand kilometres per second in the expansion velocity of the youngest remnant CasA, the detection of strong lines of oxygen from the distant remnants

in the Large Magellanic Cloud and the detailed analysis of the spectra of remnants younger and older than Puppis. These data will provide important new insights into problems ranging from the masses and structure of supernova progenitors to the structure of the interstellar medium and the nucleosynthesis of heavy elements in the Galaxy. Unfortunately, the existing data base is all that astronomers can hope to get for many years to come. With the demise of

the Einstein satellite last April, the only operating X-ray satellite is the small Japanese X-ray burst detector Hakucho. It will be over a year until ESA's EXOSAT is launched, at least 5 yr until the next American satellite and nearly a decade until the follow-on to Einstein is operational. There will certainly be time for the most thorough digestion of the existing X-ray data; one can only hope that starvation does not ensue. □

New discoveries from the interstellar laboratory

from Eva N. Rodríguez Kuiper

IN recent years the list of sulphur-containing microwave interstellar molecules has been increasing steadily. We now have seen SiS, CS, NS, SO, OCS, SO₂, H₂S, HNCS, H₂CS, CH₃SH and HCS⁺. The identification of this last molecule, the only ion in the list, is the culmination of a series of experiments performed by two groups of scientists. The first group, of P. Thaddeus and M. Guelin at the Goddard Institute for Space Studies and R. Linke at the Bell Telephone Laboratories¹, used various radiotelescopes, such as the NRAO 11 m, the University of Texas 5 m and the Bell 7 m dishes, to search for interstellar clues at 85 GHz. The second group, a team of scientists at the Department of Chemistry, University of Wisconsin in Madison², performed the terrestrial laboratory measurement which led to the confirmation of the interstellar laboratory detection.

Ion chemistry has an important role in the formation of interstellar molecules since ion-molecule reactions can proceed more rapidly than molecule-molecule reactions at the low temperatures of the interstellar medium. The identification of ions in the interstellar medium is necessary to confirm theoretical models for the synthesis of interstellar molecules. Unfortunately, microwave ion spectroscopy is difficult in the terrestrial laboratory and only in recent years have scientists succeeded in measuring the spectra of a very few of these important interstellar species.

The history of interstellar ions in the microwave band dates to the famous discovery³ in 1970 of X-ogen — an unidentified molecule seen in many sources. *Ab initio* quantum mechanical calculations⁴ suggested that X-ogen was the formyl ion, HCO⁺, and a prediction

for transitions of the isotopic ion, H¹³CO⁺ was made^{4,5}. Within a year the isotopic species was tentatively identified in the interstellar medium⁶. Finally⁷, a chemical name was given to X-ogen in 1975 by taking the microwave spectrum of the formyl ion in the laboratory and verifying the interstellar line at a frequency of 89,188.545 MHz. The isotopic H¹³CO⁺ line was also verified in the laboratory.

In 1975, CO⁺ was also measured in the laboratory⁸, but this ion was not observed in interstellar clouds until recently⁹. The laboratory spectrum of N₂H⁺, originally observed¹⁰ in the interstellar medium in 1974 and tentatively identified on the basis of a quantum-mechanical calculation¹¹, was obtained¹² in 1976. Now HCS⁺ has been added to the list of ions of astrophysical interest observed in the laboratory.

The path that led Thaddeus and his colleagues to the discovery of HCS⁺ was a search for a pattern in the source-to-source behaviour of three unidentified lines. The lines, designated A, B and C, formed a triplet at 85 GHz which has been seen in the molecular source SgrB2 in the centre of our Galaxy. The triplet formed in other sources — Orion A, DR21, TMC-1 and W3 — but the relative intensities of A, B and C showed a larger variation than could be explained by temperature effects. A search was then begun for harmonic patterns for each line by searching for additional lines at 1/2, 5/4, 4/3 and other simple ratios of the original frequencies. After a 3-year search at Texas and Arizona, they moved on to the Bell Laboratories telescope where success was finally achieved. Lines were found at 3/2B, 5/4C, 3/2C, 5/2B and 3B. These detections severely constrain the mass and structure of the molecules B and C. Line B was deduced to be HCS⁺ observed in the $J=2-1$, $3-2$, $5-4$ and $6-5$ transitions. Line C has been tentatively identified as either HOCO⁺ or HOCN.

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Line A presents a more complex puzzle yet to be solved.

Interstellar chemists have to use microwave ion spectroscopy in the interstellar medium to fill in the gaps as chemical models of the interstellar medium are poorly constrained. Synergistic observations of chemically related species are required to refine models of interstellar clouds and observations of ions are particularly important since ion-molecule reactions are a key synthetic process in the clouds. The HCS^+ ion is probably synthesized by reaction of formyl ion, HCO^+ , and ubiquitous H_3^+ ion, with CS: H_3^+ or HCO^+ + CS $\rightarrow$ HCS^+ + H_2 or CO. The major loss process for HCS^+ is recombination with electrons or reaction with O atoms: $\text{HCS}^+ + e^- \rightarrow \text{CS} + \text{H}$ and

$\text{HCS}^+ + \text{O} \rightarrow \text{OCS}^+ + \text{H}$. Simultaneous observations of HCO^+ , HCS^+ , CO and CS can therefore lead to constraints on the abundance of H_3^+ , electrons and O atoms.

There are still scores of unidentified interstellar molecules and new ones are found as telescope sensitivities improve. The unscrambling of these signals via *ab initio* calculations and clever observing techniques combined with more laboratory microwave ion spectroscopy will lead to more ion identifications. Many of the interstellar lines may turn out to be exotic highly reactive species difficult to produce on Earth but which persist for longer times in an environment where 10^6 per cm^3 is considered a dense region. The added challenge will be to confirm the identifications in Earth-based laboratories and

to measure microwave transitions for other ions which might match up with lines already observed. □

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Why study palaeoecology?

from Andrew Hill

As well as being the art of the soluble, science should be the art of the relevant. A distinction between these normally closely allied scientific activities seemed occasionally rather too evident at a colloquium on the environment of Plio-Pleistocene hominids held recently in Paris*.

Participants presented new information on a variety of fossil vertebrates and on fossil flora known from wood fragments and pollen. A few contributors arrived at more synthetic palaeoecological conclusions about fossil sites in eastern and southern Africa, using various techniques. There was even some mention of the hominids themselves, but only in the sense of placing the subject in some sort of general perspective. It was a little surprising to find practically no explicit discussion of why we might be interested in the environment of our Plio-Pleistocene ancestors; presumably a significant object is a better understanding of major human evolutionary shifts in terms of adaptation.

The main human morphological changes embody interesting evolutionary problems and it would have been valuable to try to explain them ecologically. One such problem is the origins of bipedalism, which these days might be equated with the divergence of hominids from their non-hominid ancestors. Another is dental change, presumably connected with changes in feeding, exemplified in its extreme form by the contrast between the *Homo* lineage and the very successful contemporary robust australopithecines. Yet another is the great increase in brain size during the evolution of *Homo*. How do all these relate to environmental and other ecological change in

Africa during the Plio-Pleistocene?

That these clearly identifiable problems were not squarely tackled may reflect some major methodological difficulties with terrestrial vertebrate palaeoecology as currently practised. For example, information in palaeoecology is very sparse; even where it exists its time resolution or spatial relevance is not easy to assess. Consequently there is a justifiable tendency, as at this meeting, to believe in the primacy of data, to solve any problem solvable and to believe any scrap of evidence relevant to the specific problems about which we should be concerned. In contrast, in neoecological work data are more or less unlimited and workers recognize the need to be highly selective. They consider their main problems first and secondarily judge what features of the environment might be relevant to solving them. Clearly palaeoecology would benefit from a closer identification with its modern counterpart.

But a second problem comes from too close an imitation, for palaeoecologists readily accept and try to implement the techniques of neoecology. For example, the relative numbers of different species is often an important parameter of modern terrestrial communities and so people working on fossil vertebrates attempt to calculate the relative numbers of species in their assemblages. Unfortunately fossil assemblages are not like modern communities — the nature of the material is different and probably demands different and original approaches from those used in modern ecology. Instead of examining fossil collections with neoecological methods, we should probably be looking at them in their own terms, and referring to contemporary communities from this palaeoecological perspective.

Modern analogues of environment and species behaviour are also heavily relied

upon. It seems so easy to say that the known fossil information is compatible with models reflecting modern environments that it becomes difficult to envisage ways in which past environments could differ from present ones. Do palaeoecologists even possess an adequate vocabulary to imagine anything other than savannah mosaic, more or less, wetter or drier, bush or grassland? Perhaps we need make more sophisticated distinctions if they are to have any chance of being relevant to the evolution of hominids and other creatures.

According to conventional criteria however, the meeting was a considerable success. The presentation and discussion of material possibly relevant to these more specific issues was very useful to those in the field. In addition to much new and basic taxonomic information, a biostratigraphical scheme was put forward for the important Lake Turkana sequence; relationships between east African sites and the less easily dateable sites in the south were clarified. It was resolved to collate various information for different localities and make this more generally available. The colloquium demonstrated that successful study of palaeoecology is extremely difficult, although the quality of the information collected shows that the situation is far from hopeless. However, palaeoecologists should stop sheltering behind assertions that the science is in a primitive 'data-gathering' stage; we need to get to grips with the relevant and interesting problems instead of assuming the more complacent and easy posture of acquiring more data just because it is possible to acquire it, of solving 'problems' just because they can be solved. □

*Colloque International sur l'Environnement des Hominidés au Plio-Pleistocène held in Paris at the end of June, under the auspices of the Fondation Singer-Polignac, was organized by Professor Yves Coppens, Director of the Musée de l'Homme. The proceedings of the conference will be published by the Fondation.

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NEW JOURNALS' REVIEW

Making journals fit for readers

THE general opinion that there are too many journals is annually contradicted by the appearance of yet another crop. Who is wrong — the complainants or the publishers? The conflict is, of course, semantic. There are always more journals to be read than can be read, but there are never enough outlets for contributions to the continuing torrent of discovery. Those who publish new journals are thus merely responding to demand, which is why the pages that follow set out shamelessly to provide an appraisal of many of the clutch of new journals that have appeared for the first time in a period of 30 months ending about a year ago. Some of these publications may have only a short life; others may come to occupy important and permanent niches in the scientific literature. The reviews that follow suggest that a number of the journals referred to do have a chance of making the grade.

But which? Many of the new journals differ in distinctive ways from conventional archival journals. Some, for example, seek to provide their readers with commentary and review as well as with original articles. One, at least, follows the pattern of some of the most readable of scientific journals by publishing, together with accounts of original work, criticisms by colleagues in the field, not all of them friendly. Some promise to review books, some even offer to print correspondence from readers. Others, understandably but also quite properly, are prepared to offer potential contributors rapid publication or a ration of reprints free of charge. The diversity is to be welcomed. There are signs but not statistics that more publishers than in recent years are seeking new ways with journals. That too is welcome. And even though the journals that have no obvious place in the pattern of the literature, or which appear to be badly organized, may quickly go out of business, that will not be the end of the world. For this is a field in which the survival of the fittest is a virtuous principle.

But would it not be more sensible for the scientific literature to be so organized that the costs of such failures, or even the demand for new journals, could be avoided? This is a recurrent response to the general conviction that everybody has too much to read. The response is, however, exaggerated. Schemes such as that put forward in the 1950s by the late J.D. Bernal for making available to individuals only copies of those articles in which they have declared an interest have been put forward often, but are unlikely ever to become realities. Delays in publication, always too long, are sufferable but delays in winning access to what may already have been published are intolerable. Computer-based services for information retrieval are useful adjuncts to the scientific literature, but will never be a substitute for any part of it. Electronic journals (which would replace paper with computer terminals) have not yet proved themselves. Even though, in most fields, nobody can hope to read everything, most people will also confess that they quickly learn of the publication of important articles in their fields. For the formal processes of publication are strengthened by the network of laboratory gossip.

The chances are that techniques for the electronic processing of the scientific literature will come into their own only when the technology of communications has advanced still further. As things are, most computer-based retrieval systems rely on lists of titles, with or without accompanying abstracts, which can be searched for chosen combinations of key words. Compiling the appropriate computer tapes is both labour-intensive and subjective. The ideal is that retrieval systems should be capable of storing scientific articles in their entirety, so that it is possible for potential users to retrieve not merely an author's conclusions but the nuances with which these are invariably qualified. Systems for

doing just this in more general fields are already operating on a commercial basis in the United States. Similar applications to the scientific literature will, however, have to await either the arrival of the time when scientific journals are set in type electronically (when the corresponding tapes could be fed directly into the data banks) or the development of computer techniques for reading scientific prose (mathematical equations included).

None of this implies that conventional journals, old or new, need not bother to change their ways. The arrival of electronic communication is not the only change in the environment of science in the past two decades. The scale of scientific enterprise has grown enormously, while in some fields — molecular biology is the spectacular example — the pace of discovery has become almost breathtaking. Perhaps more important, there has been a change in what might be called the texture of discovery. First, the tests of what constitutes an acceptable scientific article have become more stringent: papers are expected to be complete in themselves, to stand on their own feet. At the same time, it is no longer every author's ambition that every article published should be definitive. Loose ends abound and are described euphemistically as topics for further investigation. If openly acknowledged as such, they are left dangling in the literature. The result is that, especially in quickly moving fields, even the formal literature has a certain breathless quality.

As a consequence of the press of business and the rush to publish, especially in competitive fields, even the best managed journals can no longer be confident that their own or their referees' judgements will be well-informed as well as free from bias. Thus a journal may publish one article but inadvertently turn away another coming to similar conclusions (as *Nature* recently did with two similar papers on the bacterial expression of the haemagglutinin gene of human influenza virus), because the judgement of what constitutes a significant advance is more prone to fluctuate with time and between individuals when papers are preliminary and numerous than when they are definitive and few.

There is also an increasing number of occasions when the readers of journals dissent strongly from what contributors have to say. The two groups may share a common scientific interest, but their interests as journals' users may differ profoundly. One small but practical consequence is that journals of all kinds must be prepared to let their readers make amendments to the conclusions of articles with which they disagree. The chief offenders in this respect include some of the best-known journals published by the learned societies. Yet the example of the innovations recently introduced to *Physical Review Letters* (which now permits contributions similar to "Matters Arising" in this journal) shows that society publication is not an absolute bar to sensible policies.

There are other ways in which readers may be helped. Prudent journals will increasingly seek more positive ways of satisfying the needs of their readers. It is natural that commercial publishers, who live by subscriptions, should be the chief among the sources of innovations in the interests of readers in the pages that follow. Much, however, remains to be done. If information retrieval is a serious problem, might not specialized bibliographies be a valuable service for the readers concerned? If international conferences are an important means of communication, can even the more sedate journals continue to ignore them? And if bad grammar and other causes of obscurity are impediments to understanding, can learned journals continue to give house-room to gobbledegook? □

Specialization in the Biochemical Sciences

P.N. Campbell

IN SPITE of the economic problems of both libraries and publishers, new journals continue to be produced and nowhere among the various subject areas is this more apparent than in biochemistry and molecular biology. Leaving aside the publisher who adopts a policy of "suck it and see", in that he spawns new journals comparatively irresponsibly on the basis that at least a proportion will succeed, there are a variety of sound reasons for the continued production of new journals.

Among the more compelling of these is the need to package research papers and reviews in a way that attracts subscribers. The large general journals continue, but in order to survive they must attract contributions on an international basis and they must be large for otherwise a reader is unlikely to find in any one issue something of interest. The same probably applies to general journals specializing in the rapid publication of short papers. Because of their size the successful general journals are bound to command a high subscription price; for this reason and because the individual reader finds that only a very small proportion of the papers are of interest to him, there has been a demand for more specialist journals, aimed at a more circumscribed readership.

The new publications covered in this review are all specialist, although the tag may be interpreted in two ways. First, there are those which are focused on a specific sub-discipline (or, as its practitioners might like to think, emerging discipline). Second, a larger group attempt to set up new areas of specialization by encouraging contributions which take account of research in different areas.

Both can be laudable aims. But although a new journal of traditional concept may get by, even be successful, one also looks for the application of fresh ideas to the tangled question of taking the right information to the right people. If nothing else, these 13 titles are, taken together, a manifestation of the virility of the publishing industry. Sadly, though, that extra ingredient — imagination in their approach to their subjects — is largely absent.

Perhaps the most unusual of all the newcomers is the *Key Reports in Cell and Molecular Biology*, part of "an integrated publication system", *IRCS Medical Science*. Some 32 specialist journals covering selected disciplines provide a medium for the rapid publication of concise accounts (1,000 words, or so) of original results from all areas of biomedical research. The two *Key Reports* (the other is

IRCS Medical Science: Key Reports in Cell and Molecular Biology. Editor M.R.J. Morgan. 12/yr. (Elsevier-IRCS, Lancaster, UK.) £50 UK, \$125 elsewhere. *Prostaglandins and Medicine*. Executive editors D.F. Horrobin and M. Karmazyn. 12/yr in 2 vols. (Churchill Livingstone.) £80, \$180. *Cell Calcium*. Editor R.M. Case. 6/yr. (Churchill Livingstone.) £40, \$90. *International Journal of Biological Macromolecules*. Managing editor Stella Dutton. 6/yr. (IPC Science & Technology Press.) 1981: \$195 US, £75 UK and elsewhere. *Enzyme and Microbial Technology*. Editors J.F. Kennedy and O.R. Zaborsky. 6/yr. (IPC Science & Technology Press.) 1981: \$46.80 US, £18 UK and elsewhere (personal); \$156 US, £60 UK and elsewhere (institutional). *Biological Trace Element Research*. Editor-in-chief G.N. Schrauzer. 4/yr. (Humana.) \$80. *Cell Biophysics*. Editor N. Catsimopoulos. 4/yr. (Humana.) \$80. *Journal of Biochemical and Biophysical Methods*. Managing editor C.F. Chignell. 12/yr in 3 vols. (Elsevier/North-Holland Biomedical.) Dfl.606. *Membrane Biochemistry*. Editors-in-chief A.E. Shamoo and G.F. Sachs. 4/yr. (Crane, Russak.) \$70. *Journal of Receptor Research*. Editor D.F.H. Wallach. 6/yr. (Dekker.) \$37.50 (personal); \$75 (institutional). *Calcified Tissue International*. Editor-in-chief L.V. Avioli. 6/yr. (Springer-Verlag.) DM 480. *Journal of Muscle Research and Cell Motility*. Editors R.T. Tregear and C.C. Ashley. 4/yr. (Chapman & Hall.) £23.50 UK (personal); £46.50 UK, £56 elsewhere (institutional). *Muscle and Nerve*. Editor W.G. Bradley. 9/yr. (Wiley.) \$70 US, \$85 elsewhere (personal); \$80 US, \$95 elsewhere (institutional).

Human and Animal Physiology) contain a monthly selection of about 20 papers considered to be of particular significance, and each covers a broader field than any one of the specialist journals.

In this system, speed is of the essence, and publication is certainly rapid, papers taking only four to eight weeks from receipt to appearance. For readers, the limitation on length of contributions is attractive since one can rapidly get the gist of the story. Yet the drawback would appear to be that the back-up material will be difficult to trace (if, indeed, it is published at all) and I suspect that the best research work will not appear in this form — at least not until the economic crisis forces it. Nevertheless this is an interesting

system and possibly an indication of future trends.

Another innovation, albeit less encompassing, characterizes the two Churchill Livingstone publications covered here — *Prostaglandins and Medicine* (PM) and *Cell Calcium* (CC). At



the end of each issue, both publish a bibliography on their respective areas of interest which is prepared by the University of Sheffield Biomedical Information Service. This is doubtless a useful service to readers and one that could with profit be adopted elsewhere.

PM aims to publish all types of work on prostaglandins, studies related to medicine being particularly welcome, and contributions appear in the form of research papers, reviews and case reports. The arrival of PM means that there are now two journals to service this field, the other being the already established *Prostaglandins*; there seems to be room enough for the two of them, since both contain good papers.

Like its stablemate, CC hopes to attract contributors and subscribers from both pure science and medicine. The journal carries original research papers and reviews, and its succinct title sums up its scope — the regulation of cell calcium itself and the regulation by calcium of cell function.

Apart from the difference in subject matter, a large gulf in price separates PM and CC. The two journals are both camera-ready productions (CC being of a slightly higher standard) and both are due to appear monthly during 1981, but the higher cost of PM over the year is justified by the comparatively large numbers of papers included in each issue (12–14 compared with 5–7 in CC). Layout in both journals is somewhat over-generous; for example in the June 1981 issue of PM there is a lot of space wasted by figures which could have been replaced by a statement that "no significant alteration in —ase activity was observed from control values at any of the calcium concentrations examined" (pp.635, 636).

In this collection of new journals, the two IPC journals stand out immediately by the quality of their production. Both would not be out of place on a coffee table perhaps not surprisingly, because the *Inter-*

national Journal of Biological Macromolecules (IJBM) and *Enzyme and Microbial Technology* (EMT) come under the imprint of a publishing house more famous for women's magazines than science journals.

The object of IJBM is to publish articles on all aspects of the structure, function and interactions of biological macromolecules and their assemblies. Contributions appear in the form of original research papers, preliminary communications and "Notes to the Editor", as well as in a correspondence column. There is little doubt that the various studies covered by the umbrella title of "biological macromolecules" would benefit from cross-fertilization, but I wonder whether there is enough cohesion in papers covering carbohydrates, nucleic acids and proteins to attract contributors (and subscribers) in the face of competition from the more specialist journals.

EMT is also interdisciplinary in aim, and beautifully produced in a large format. The journal was launched to provide a forum for the coordination of biotechnology and bioaffinity, especially the directed use of enzymes and microorganisms. Research papers, short papers and review articles are accepted. The growing importance of this field is unquestionable, but it involves many scientists and engineers from diverse backgrounds. Only now are they learning how to work together, and EMT should help them to talk to each other. The journal is certainly timely and I understand that it is proving to be a very successful venture.

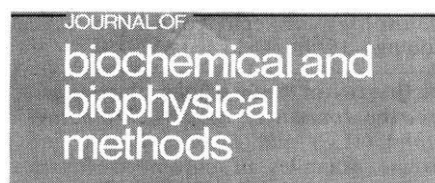
Providing channels of communication between isolated fields is evidently seen by publishers as a boom area for the 1980s. Yet another interdisciplinary journal is *Biological Trace Element Research* (BTER), the aim of which is to emphasize the integrative aspects of trace metal research in all appropriate fields. The journal publishes regular papers and reviews with a reasonable time-lag of about five months. Undeniably, this is an expanding area of research and one which calls for papers that are written with a wide audience in mind. That this is not always achieved is best exemplified by finding papers entitled "Epidemiological Study of Chromium Platers in Japan" and "Chromate Metabolism in Liver Microsomes" virtually cheek by jowl in the journal. Nonetheless, BTER is a well-produced journal that may fulfil a useful role in a developing area of toxicology.

Also well produced and from Humana Press comes *Cell Biophysics* (CB). The journal carries original papers, and occasional reviews on the key experimental, methodological and theoretical investigations on cells and their principal organelles and subfragments by biophysical methods. The aim is to publish studies at the cellular rather than the biochemical level.

While I can appreciate the general objective, in that cell biology is of much current interest now that we have such excellent means for examining cellular events, I would not have thought that the inclusion of "biophysics" would be an attractive feature. In my terms, CB is not a specialist journal, for the range of subject matter is so broad that each issue needs to contain many papers if it is to attract, and hold, attention. Thus far CB has not published a sufficient amount of material to make good this inherent drawback in its concept.

The scope of the *Journal of Biochemical and Biophysical Methods* (JBBM) is, in its different way, as broad as that of CB. Yet I would guess that the future of this journal is assured from the contributors' side — there is generally no dearth of papers for a methods' journal. The role that JBBM hopes to take is simply expressed in its title, and the journal publishes regular papers, short notes, short reviews and book reviews. Publication is fairly rapid, some five months after acceptance.

I should confess a bias against methods' journals. Biochemists do not specialize in techniques *per se* but in areas of science; at the same time, although rarely exciting, methods do of course provide the backbone to experimental research. My



doubt about JBBM lies in the fact that the papers in the four issues available to me in the main report modifications of existing practice. One would have thought that to thrive, rather than merely survive, the journal needs to poach genuine "break-through" papers from the large general or specialist journals.

An initial (and vital) step in the launching of a new journal is the gathering of names for the editorial board. In part this is an exercise in window dressing — convincing the potential subscriber that the journal has a standing by association with the renowned in his field. In common with most of the journals covered here, *Membrane Biochemistry* (MB) has assembled an impressive board of some 40 scientists of widely scattered affiliations. An increasing number of biochemists and biophysicists regard themselves as membraneologists, so here we have a journal which should satisfy a reasonably homogeneous group of readers. MB publishes straightforward research papers but there is no mention of dates of acceptance, so speed of publication cannot be gauged.

This area of science is popular and there are plenty of papers to fill the pages of its journals. However MB will have fierce competition for subscribers, and, although well produced, it is not a cost-effective pro-

New Journals' Issue

THE brief given to the authors in this issue was to limit themselves to comment on the journals sent to them for review. Reviewers were asked to avoid detailed discussion of general questions of periodical publishing and information systems. In the following pages, then, will be found description, comparison and criticism of some 120 journals, but little consideration of the wider aspects of the primary literature.

The criteria used to decide whether or not a given publication should be included in this issue were that:

- (i) the first number appeared, or the journal was re-titled, between January 1978 and May 1980 (a few exceptions have been made to this rule);
- (ii) it is published at least three times a year;
- (iii) the main language used is English.

Thus, broadly, periodicals of professional interest to scientists were considered for review, with the exception of abstracts' journals. The cut-off date of May 1980 was chosen to allow enough issues of a journal to have been published, so as to give reviewers a reasonable sample of material on which to base a judgement.

To many (though probably not publishers, subscription agents and librarians), the number of new titles appearing over this period may come as a surprise. Yet many journals submitted for review are not covered here. Some of these are doubtless worthy publications, and their omission implies no pre-judgement of their value. A further consequence of constraints on space in this issue is that the shortest possible (unconventional) title abbreviations have been used in the reviews.

Where possible, reviewers were asked to cover a number of broadly related journals, but in many instances this was impracticable. The preponderance of titles in the biological sciences reflects the bias of the material sent in for review and, presumably, a current trend in journal publishing.

Publishers and journal sponsors were requested to submit four issues — the first, the most recent and two others — of each title for review (in some cases fewer were available). Opinions expressed in the reviews are not based on a full back-run of a journal, but on a sample, and apply to mid-1981 at the latest. The details of editors and frequency of publication, and the basic annual subscription prices appearing at the top of each article are given, where possible, for 1982. These details are taken from information supplied by publishers or from back issues, and are not necessarily complete. *Readers should check prices, in particular, with the publisher concerned.*

An index to the journals reviewed appears on p.369.

CELL BIOPHYSICS

An International Journal

position; in 1981 it looks as though the journal will publish only 20 papers at a cost of \$70.

Rather better value for money is the *Journal of Receptor Research* (JRR) from Marcel Dekker. Especially well put together (Dekker have shown that camera-ready publications need not be ugly) this journal publishes research papers and mini-reviews with a focus on laboratory and clinical research on biological receptors. However the same question must be asked of JRR as of IJBM and CB — is there really sufficient cohesion in the subject matter? Perhaps the late appearance of Vol.2, No.1 (1981) reflects this doubt.

One can be more certain about the prospects of *Calcified Tissue International* (CTI), a journal which underwent metamorphosis in 1979 from *Calcified Tissue Research*. CTI reports research emphasizing the structure and function of bone and other mineralized systems in living organisms. It accepts original studies, rapid communications, comments and reviews; in addition, abstracts of papers presented at meetings — for example those of the American Society for Bone and Mineral Research — appear occasionally.

The progenitor of CTI, started in 1967, was well established. This fresh form is excellently produced and covers a clearly delineated field — it should continue to do well.

Finally, two journals dealing in their different ways with muscle. The brief of the *Journal of Muscle Research and Cell Motility* is all aspects of muscle, contractile mechanisms and cell motility. Long and short research papers, short reviews, conference reports and book reviews are published. This is an excellent journal with regard to the quality of the editorial board and contributors, and to the standard of production. It deserves to thrive.

Muscle and Nerve (MN), covering research into neuromuscular disease, is medical in orientation and is supported by the Muscular Dystrophy Association. The only issue available to me contains an editorial, main articles, notes on new developments, abstracts of a meeting, letters to the editor, book reviews and a calendar of events. The diversity of the contents will provide interesting reading for subscribers and MN would appear to provide a useful "talking shop" for a well-defined group of research workers.

In judging this array of new journals, I feel rather like a nurse at a baby clinic who does not wish to assess the chances of survival and development of the infants in front of the parents. It has been a salutary experience in that I now have a healthier respect for the enterprise of the publishing industry, and a great deal of sympathy for the librarians who have to deal with the continual, confusing flux of journals. In addition, two points have become evident.

First, the variation in format of these

young journals is staggering. In part, the reasoning behind this diversity is perhaps (for example) that the glossies of IPC are intended more for the industrial laboratories and the modest camera-ready journals of Churchill Livingstone are designed for the academic research laboratories. However, I have to admit that I am inclined against camera-ready copy unless the authors can be disciplined and the photographic processes are carefully done as with the *Journal of Receptor Research*. Surely many other scientists will have the perhaps irrational feeling that a professional-looking journal is more likely to have contents worth consulting.

Second, if all these journals survive, the corollary for me is that the system whereby one has a central library that takes all the

SEPARATION SCIENCE AND TECHNOLOGY

relevant journals will break down. The central library will concentrate on the good, international, general journals in each subject, the departmental libraries and individuals taking the specialist journals and looking upon them as ephemeral literature. □

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New Media for Separation Techniques

Ivor Smith

Journal of Liquid Chromatography. Editor J. Cazes. 14/yr + supplements. (Dekker.) \$236. *Electrophoresis*. Editor-in-chief B.J. Radola. 6/yr. (Verlag Chemie.) DM 176. *Separation Science and Technology*. Executive editor J.C. Giddings. 15/yr. (Dekker.) \$379.50.

THE first two of the three journals reviewed here, the *Journal of Liquid Chromatography* (JLC) and *Electrophoresis*, are extreme examples of current trends in science periodical publishing.

Membrane Biochemistry

The scope of JLC is very broad, covering the whole field of separation science; one issue, for example, contains papers on topics as diverse as HPLC of petroporphyrins, TLC of Cu/Hg chelates and the quantitative determination of acenocoumarin in anticoagulated patients. Hence one would find, at most, one or two papers of interest in each issue. This may be partly compensated for by the single-topic issues such as those on TLC, and HPLC of aminoacids-peptides-proteins.

Conversely, *Electrophoresis* is a very "tight" journal. Although ostensibly devoted to a technique of wide application which uses many supporting media, in practice it restricts itself almost entirely to one- and two-dimensional gels using "classical" acrylamide separations, IEF and isotachopheresis as applied to proteins and peptides. If this encompasses one's interests then every page would be valuable and one would want to publish in it as well. I would think that the scope of this journal

is likely to expand to include nucleic acid and nucleotide sequencing, as the techniques are frequently identical.

It is difficult to see for what new audience JLC is intended, or what gap it is supposed to fill. This field is already well served by the *Journal of Chromatography* and all of the papers in JLC could have appeared there. The new outlet may speed up the publication of papers on the subject, however. In contrast *Electrophoresis* certainly appears to fill a need. Indeed, those not in the field might well encourage those who are to publish in it in order to remove the ever-increasing number of such highly specialized papers from the more general journals. Both journals are clearly poor value for money, as can be seen by the fact that in the whole of London University only one college takes *Electrophoresis* and no one takes JLC.

The other title covered here, *Separation Science and Technology* (SST), is a continuation of an established journal,

JOURNAL OF LIQUID CHROMATOGRAPHY

Separation Science. Like its predecessor, SST adopts a highly mathematical, physico-chemical approach to the engineering and technological aspects of large-scale separation. The change of title allowed a broadening of approach whilst retaining the original editorial intention. Both London colleges known for their interest in the industrial aspects of separation take it; they should continue to find it useful. □

Ivor Smith is at the Courtauld Institute of Biochemistry, Middlesex Hospital Medical School, London.

Fertile Fields

R.J. Aitken

Gamete Research. Editor R.B.L. Gwatkin. 8/yr in 2 vols. (Alan R. Liss.) \$120 US, \$142 Europe. **Archives of Andrology.** Editor-in-chief E.S.E. Hafez. 8/yr in 2 vols. (Elsevier/North-Holland Bio-medical.) \$208. **Reproduccion.** Editor-in-chief J. Cortés-Prieto. 4/yr. (MTP Press, Lancaster, UK.) £20 (personal); £35 (institutional).

Gamete Research (GR) was conceived with the aim of creating a channel of communication between scientists working in the general areas of gamete biology, fertilization and embryonic development in different phyla, even different kingdoms. Thus the papers cover such topics as sperm ultrastructure, sperm motility, oocyte maturation, fertilization and cleavage in a diverse range of species including ferns, crustaceans, echinoderms, amphibians and mammals, including man. Book reviews and useful review articles also appear, and the journal maintains a good standard of production.



For reproductive biologists working exclusively with mammals this newcomer is particularly welcome, since conventional outlets for their speciality such as the *Journal of Reproduction and Fertility* and *Biology of Reproduction* do not encourage the comparative approach and so rarely publish articles on non-mammalian species. The high quality of research appearing in GR has been particularly evident of late, perhaps indicating a progressive increase in the popularity of the journal. It can certainly be recommended.

Also reflecting the growth of distinct subdisciplines in reproductive biology is the appearance of *Archives of Andrology* (AA), a clinically directed journal specializing in male reproductive physiology. The journal publishes a balanced mixture of scientific papers dealing with anatomical, physiological and biochemical aspects of male reproduction in animal models, and very clinically orientated articles reporting on such topics as sexually transmissible diseases and the characterization, diagnosis and treatment of male infertility. In addition to research papers and short communications, AA publishes review articles which I found to be one of the strongest aspects of the journal. In contrast, the research papers are of variable quality and in this respect the journal does not score as highly as its main competitor, the *International Journal of Andrology*.

AA is professionally put together, and

any clinically orientated department wishing to cover this rapidly developing field should certainly take it. However, those predominantly interested in the scientific aspects of male reproductive physiology might be better served by the competition.

Yet another journal of reproduction to appear recently, or rather re-emerge under a new imprint, is *Reproduccion*, the official organ of the International Federation of Fertility Societies and the International Academy of Reproductive Medicine. The journal is aimed at a broad spectrum of readers covering a wide range of clinical topics (andrology, gynaecology, sociology) as well as the science of reproduction in animal models, including sub-mammalian species. As an illustration of the variety of interests covered, a recent issue contained papers on oogenesis in *Bufo arenarum*, human uterine contractility in the puerperium and endometrial bacterial flora following the insertion of an IUD.

The geographical bias of *Reproduccion*



is reflected in the fact that about 50 per cent of the articles are written in Spanish or Portuguese (with English summaries) while the remainder are in English. Its real purpose, then, is to act as an important outlet for scientists working in the reproductive sciences in Spain, Cuba, Mexico and other Latin American countries, and it is in such locations that the primary market for the journal must lie. *Reproduccion* contains some quality science and, for the sake of those not conversant with Spanish and Portuguese, is covered by all of the major indexing and abstracting journals. □

R.J. Aitken is at the MRC Unit of Reproductive Biology, Edinburgh.

Developments in Physiology . . .

David Yudilevich

Journal of Developmental Physiology. Editor C.T. Jones. 6/yr. (Blackwell Scientific.) £50 UK, \$138 US, £60 elsewhere. **Placenta.** Editors H. Fox and W. Page Faulk. 4/yr. (W.B. Saunders.) £32.50. **American Journal of Physiology: Endocrinology and Metabolism.** Editor E. Knobil. 12/yr in 2 vols. (American Physiological Society.) \$55 US, \$70 elsewhere. **American Journal of Physiology: Gastrointestinal and Liver Physiology.** Editor L.R. Johnson. 12/yr in 2 vols. (American Physiological Society.) \$50 US, \$65 elsewhere.

It is almost a truism to remark that a certain area of science is expanding. Yet, of course, the statement applies to many aspects of physiology and here, in the *Journal of Developmental Physiology* (JDP) and *Placenta*, are two new journals making manifest such growth in the study of fetal development.

Publication of research in this field was previously scattered through a large number of unrelated journals, particularly in the fields of obstetrics and paediatrics. The intention of drawing together this material in *Placenta* and JDP (which overlap to a certain extent), with the additional hope of stimulating research in these fields, seems to be justified.

In the opening editorial of *Placenta*, the new journal is put in context: "the fecundity of medical and scientific journals is quite alarming . . . the lack of a forum for placental studies is glaringly apparent". However, although the editors express a wish to publish material from

"workers in an unusually wide field of interest and disciplines", to date this aim has not been fully achieved since a large number of the papers are morphological studies of a clinical nature. The quality of production of the journal is excellent; the papers are well laid out and many contain a generous number of beautiful micrographs. The same quality has been extended to the hardback supplements (two so far) which are the proceedings of conferences.

Disappointingly, the quality of some of the papers in *Placenta* is below average; a few are so short as to resemble expanded abstracts padded out with a few large figures and there is sometimes an unnecessary subdivision of material into several papers. Other editorial weaknesses are apparent; for example, some papers lack a summary and manuscripts "in preparation" are included in the reference list, a bad practice in my view.

The Guardian (22 January 1980) in a prominent article emphasized that the human placenta is "one of the most promising experimental animals of the future". A view from a different perspective was given by R.D.H. Boyd in a review in *Placenta* on the topic of transport of substances across the placenta: "there must be few fields of classical physiology as poorly understood as this one". Thus the problems of investigating processes in the placenta are great, but so is the potential. I hope that *Placenta* will stimulate sound scientific research in the entire field.

JDP aims to publish "original research

on any aspect of the scientific study of the pregnancy, the fetus and the neonate of man or of experimental animals . . .". Full-length papers, short communications and invited reviews are accepted. In the inaugural issue the editors mention that "progress in animal development has not yet received the attention given to cell physiology or molecular biology" and they encourage a modern approach to this "previously largely descriptive discipline". It is encouraging to see that methods described in the papers they have published up to now generally satisfy this criterion. Editorial policies are strict and, interestingly, at the end of the first year of publication the panel of reviewers was



actually named, and the number of papers received (76) and the proportion accepted for publication (46 per cent) was given. The journal, which is also well produced, is behind schedule (the issues of December 1980 and February 1981 were sent together in July). If the reason for this delay is the lack of suitable manuscripts, one would like to encourage scientists in the field to patronize JDP in view of its high standard so far.

The other two journals covered here, *Endocrinology and Metabolism* (EM) and *Gastrointestinal and Liver Physiology* (GLP) are not really new but are the result of a further reorganization of the old and venerable *American Journal of Physiology*. In 1977 the first subdivision of the journal was launched in five sections, one of which — *Endocrinology, Metabolism and Gastrointestinal Physiology* — contained both of these topics.

After this further fission, the editor of GLP was able to declare that this is "the appearance of the only journal devoted exclusively" to these topics. The editorial board was strengthened by the addition of several well-known Europeans, and GLP shows every sign of being successful. The rejuvenation stimulated by the subdivision is also shown in the promise made by EM that replies to submitted manuscripts will be made within five weeks and papers will be published with delays of not more than five months. Certainly, it is apparent that both of these journals have maintained the high standards of the parent publication.

There are advantages to this process of subdivision as long as the sections continue to be produced in a single monthly issue. This will ensure the integrity of physiology as a basic discipline and counterbalance the all-too-common tendency towards overspecialization. [1]

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. . . and in Psychology . . .

Albert Yonas

Journal of Applied Developmental Psychology. Editor I. Sigel. 3/yr. (Ablex, Norwood, New Jersey.) \$20 (personal); \$49 (institutional). *Infant Behavior & Development*. Editor L.P. Lipsitt. 4/yr. (Ablex, Norwood, New Jersey.) \$22.50 (personal); \$59 (institutional). *International Journal of Behavioral Development*. Editor F.J. Mönks. 4/yr. (North-Holland.) Dfl.155, \$62 (personal); Dfl.180, \$72 (institutional).

THE volume of research in developmental psychology has expanded enormously in the past 15 years. Although it is unlikely that the field as a whole will sustain this growth, one part of it that will probably continue to do so is the application of behavioural science research to social problem-solving. One expression of that expansion is the appearance of the *Journal of Applied Developmental Psychology* (JADP).

The journal's stated goal is to encourage "the synergistic relationship between applied and basic research"; however, the creation of this new journal actually may have the opposite effect. It seems something of a paradox to expect a journal specializing in applied work to help bridge the gap between applied and basic research. Nevertheless, the journal does provide a home for intervention studies that might not be published elsewhere.

Although JADP accepts reports that define social policy issues, the number of this type of paper found in the first volume was negligible. The papers that were published in the first year were quite similar in topic to those found in the traditional child developmental journals, such as *Child Development*, the *Journal of Experimental Child Psychology* and *Developmental Psychology*, and their quality was uneven. Although the subscription rate is reasonable, it appears unlikely that JADP will become a primary source of information for either researchers or practitioners in this field.

One can be more sanguine about the prospects of the other Ablex journal

explosion in the number of psychological studies of human infants.

IBD is the only English-language journal devoted exclusively to infancy, and it clearly fills a need. A growing lag in publication time of accepted manuscripts (in spite of the journal's 70 per cent rejection rate) necessitated additional pages in the last issue of Vol. 3.

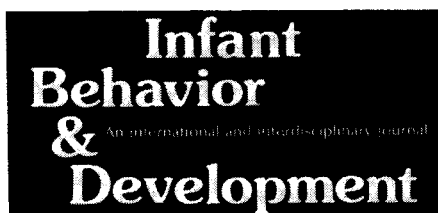
The articles that have been published are generally of high quality. The editors insist that data reported should be replicable and that findings should have some theoretical implications. Although research on non-human infants is accepted, authors of such articles have apparently found other journals better suited to their needs. Theoretical and review articles are also accepted, and book reviews by the editor appear in each issue. IBD is not expensive and should attract an increasing number of subscribers; individuals will find it particularly good value.

Notably more expensive than the other two journals covered here (but better produced) is the *International Journal of Behavioral Development* (IJBD), the official journal of the International Society for the Study of Behavioral Development. In further contrast, the journal has a broader geographical appeal than either JADP or IBD which are both largely intended for North American authors and subscribers.

While the contents of the first four volumes of IJBD represent a wide spectrum of countries, only papers written in "impeccable" English are considered for publication. Although the journal primarily contains reports of original research across the life span, it also accepts comparative studies of development, as well as literature and book reviews. The journal has attracted many high quality papers, but the methodological rigour of the studies is variable (perhaps reflecting the journal's international character), and the editors have allowed a fair latitude in theoretical speculation.

While the journal does not focus on any single new sub-discipline or cross-discipline, it does serve to encourage international communication. There are surprisingly few studies of development from a comparative or cross-cultural perspective to be found in the journal; rather, the content reflects the same concern with cognitive development that is presently found in established developmental psychology journals. Given such competition, it is at present unclear whether IJBD will attract sufficient high-quality research to become a standard resource for developmental psychologists.

Albert Yonas is a Professor of Child Psychology in the Institute of Child Development, University of Minnesota.



reviewed here, *Infant Behavior & Development* (IBD). Infancy has always been central to both nativist and empiricist accounts of mental development, but until the last decade such theories were, for the most part, unconstrained by reliable and detailed data on the behaviour of actual human infants. This situation is rapidly changing; the past ten years have seen an

... and in Neuroscience

R. Victoria Stirling

Developmental Neuroscience. Editors-in-chief A. Vernadakis and E. Giacobini. 6/yr. (Karger, Basel.) SwFr.130.20, DM 162.40, \$74.20 (personal); SwFr.186, DM 232, \$106 (institutional).

IN THE absence of any editorial statement in the issues available to me, the objectives of *Developmental Neuroscience* (DN), a new general journal of developmental neurobiology, must be judged from its contents. Unfortunately, it is not clear from a survey of the papers in Vol. 3 (1980) what is the direction or editorial policy of this newcomer.

Most papers are well-written and coherent, many of them confirming earlier work rather than breaking new ground. Only three are concerned with my own research interest, the development of patterned neuronal connections; most of the rest are biochemical in nature, discussing the regulation of brain function and metabolism by hormones, under-nutrition and ageing. At the other extreme are a few purely morphological papers, describing maturation of synaptic endings in the vestibular nucleus of the chicken and the use of immunofluorescent methods in the study of neurofilaments in early development.



The papers are almost all 10 pages long (including the well-reproduced illustrations), doubtless the result of a \$105 excess page charge. Exceptions appear to be made, however; a very full description of the structure of thyroxine-induced infrapyramidal mossy fibres in rat is 18 pages long, with an abstract of 24 lines (the stipulated length is 10 lines), perhaps because both authors are on the editorial board! Speed of publication appears to be much the same as other journals in this area, but no dates of acceptance appear with the papers.

While the birth of specialized journals is justified by the recent mushrooming of developmental neurobiological research, there does seem to be an urgent need for yet another general journal. The combination of the last three parts of Vol. 3 in one issue may indicate that DN has not yet found its niche.

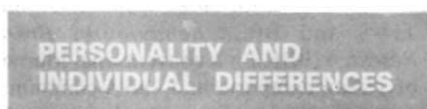
R. Victoria Stirling is at the National Institute for Medical Research, Mill Hill, London.

Trait Psychology Fights Back

Chris Brand

Personality and Individual Differences. Editor-in-chief H.J. Eysenck. 4/yr. (Pergamon.) \$65.

WITH the break-up of the *British Journal of Social and Clinical Psychology* into its named components, Hans Eysenck has filled the gap that resulted for differential psychology in general and for the London School in particular. So far, after some six high-quality issues of scholarly, empirical reports in *Personality and Individual Differences* (PID), the evidence is that, despite a decade of environmentalism and situationism in psychology, trait approaches to human differences are still alive and kicking.



Undoubtedly the outstanding report to have been carried is that of D.E. and A.E. Hendrickson, "The Biological Basis of Individual Differences in Intelligence"; but this is complemented by other physiological enquiries — particularly concerning Eysenck's third dimension of personality,

psychoticism, and related traits of impulsiveness and sensation-seeking. Naturally, many of the papers are addressed to whether or not Eysenck has got matters right: but, across large areas of differential psychology, this is by now a helpful catch-all question that researchers can reasonably be expected to consider; and contributors to PID are evidently not required to agree about the answer. Above all, there is a healthy proportion of distinguished contributors; and the journal is catholic in the psychometric measures reported, in the subjects studied and in the countries from which the reports originate.

In general, the sound empiricism of the journal allows one to tolerate some peculiar "findings" — can London's IQ really be lower than Warsaw's? — that have perhaps slipped through the refereeing net in these early days. And the harmless indulgence of the book reviews, in which an imperious view of the world as seen from Denmark Hill is offered as entertainment to some readers and as a caution to others. □

Chris Brand is a Lecturer in the Department of Psychology at the University of Edinburgh.

Butterworths

Publishers of books and journals in law, science and technology, medicine and the social sciences announce the following new journals for 1981/82.

- **Applied Geography**
- **Communist Affairs**
- **Crop Protection**
- **Electoral Studies**
- **Information Technology**
- **International Review of Law and Economics**
- **International Reviews in Physical Chemistry**
- **Journal of International Money and Finance**
- **Monumentum**
- **Museum Management and Curatorship**
- **Political Geography Quarterly**
- **Review of International Studies**
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- **Social Science Information Studies**
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Innovations in the Neurosciences

Oliver Braddick

Trends in NeuroSciences. Staff editor D. Bousfield. 12/yr. (Elsevier/North-Holland Biomedical.) £20 UK, \$45 US (personal). **The Behavioral and Brain Sciences.** Editor S. Harnad. 4/yr. (Cambridge University Press.) 1981: £24 UK, \$44.50 US (personal); £48 UK, \$89 US (institutional). **Behavioural Brain Research.** Editor-in-chief I.S. Russell. 12/yr in 3 vols. (Elsevier/North-Holland Biomedical.) Dfl. 606.

THESE three new journals explicitly place themselves in the field of neuroscience as a whole, cutting across the boundaries of traditional disciplines. Two of them are also untraditional in that they depart from the standard form of the research journal.

The general form of *Trends in NeuroSciences* (TINS) can most easily be described as *Nature*, restricted to neuroscience, minus most of the classified advertisements and all of the original research papers and letters. In its first two years its editors have apparently tried various organizations of its contents, sometimes under slightly whimsical headings such as "Nervous Connections" and "Neuroviews", but its purpose and style have been evident throughout. It has consistently carried news, meeting reports

and book reviews, but its major part each month contains clear and authoritative short reviews of current research, written by the active scientists concerned, at a level and depth which can be read with pleasure and education by scientists in other areas of neuroscience (and outside it). The main emphasis has been on functional neurophysiology, neuroanatomy and neurochemistry. There has been a deliberate policy to make clinical connections with neurology and psychiatry, and the journal can be recommended to clinicians who want to keep in contact with relevant



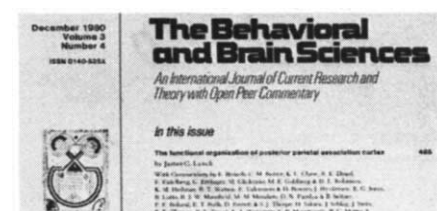
advances in the basic sciences. So far, behavioural contributions to neuroscience seem to be rather under-represented.

Many scientists will feel that their subscription allowance is better spent on a year of TINS than on an archival research journal of which they skim maybe 25 per cent in a conscientious month. The idea is clearly one whose time has come; we have *Trends in Pharmacological Sciences* and *Trends in Biochemical Sciences*, also from Elsevier. If it spreads (*Trends in Earth Sciences?* *Trends in Physical Biology?*) it may even represent a fragmenting threat to the general scientific news and research journals such as *Nature* and *Science*.

The Behavioral and Brain Sciences (BBS) makes a quite different departure from the standard journal form, which it calls "Open Peer Commentary". Each paper, following an ordinary refereeing process, is sent out to a large number of potential commentators, of whom typically about 30 return commentaries of up to 1,000 words. The original author then produces a response to the commentaries, and the whole "treatment" — "target article", commentaries and response — is printed in the journal. There is also an opportunity for "continuing commentary" in the subsequent issues, but (perhaps mercifully) this seems to have been rather little used. Although the process sounds cumbersome it appears to run smoothly and has often produced important articles and, of course, fascinating interchanges. The experience is like being present at a rather lively colloquium; the points of dispute are highlighted if not always resolved, the styles and positions of the participants are exposed, hobbyhorses are sometimes ridden with great vigour, and mutual in-

comprehension is occasionally made very conspicuous. Like remarks in a colloquium, commentaries are often incisive, integrative or bring highly relevant new information to bear on the subject, but sometimes serve principally to advertise the commentator. Since there is little opportunity for refereeing commentaries, the continuing success of the project must depend on the shrewd selection of invited commentators, and on the journal maintaining the quality which encourages good invited commentators to respond: so far it has done well.

As might be imagined, BBS's opening for commentary and disputation encourages a rather different range of scientific topics than does the reportage of TINS. Cognitive science (the meeting of psychology, linguistics and artificial intelligence) and sociobiological approaches to human ethology have featured prominently, and the opportunity for a dialogue between neuroscientists or psychologists and philosophers has been taken up on several occasions. However, the issues debated have not necessarily been general rather than technical, nor have they necessarily been more orientated to behaviour than to the brain; for example, the development of hemispheric specialization, the use of sensory feedback in voluntary movement and control mechanisms for drinking, have all been the subjects of treatments. Several articles have been condensed from current books (by Chomsky and Jensen, for example); these are sometimes billed as "multiple book reviews" but the form is not distinguishable from regular treatments.



The field covered by BBS has often suffered in the past from the drawing of battle lines between prematurely hardened positions: nature v. nurture, cognitive v. behaviourist, biological v. cultural causation. The form of BBS, used uncritically, could encourage these adversary dramas, but with intelligent selection of articles and commentaries it could be a uniquely good forum for laying bare their shaky foundations.

In contrast with the new approaches of TINS and BBS, *Behavioural Brain Research* (BBR), is a conventional journal of research reports and short communications in the familiar form. A number of existing journals (for example the *Journal of Comparative and Physiological Psychology*, *Physiology and Behavior*, *Physiological Psychology*) cover nominally similar ground. However, all of these seem to be perceived as psychologists' journals, with a side effect being a prepon-

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derance of papers on the laboratory rat. BBR has attracted a wider range of contributors and of species, in keeping with its declared aims of being both interdisciplinary and comparative. It has also established a very effective balance between continental European and Anglo-Saxon contributions, although all papers are in English.

The papers in BBR could probably all have found quite suitable homes in previously existing journals, but it is quite clear that the new journal has its own distinctive flavour, and may serve as a focus for a particular growing international and interdisciplinary research community. □

Oliver Braddick is a Lecturer in Experimental Psychology at the University of Cambridge.

Methods' Reading

D.W. Richter

Journal of Neuroscience Methods. Editor-in-chief J.S. Kelly. 8/yr in 2 vols. (Elsevier/North-Holland Biomedical.) Dfl. 404.

THE editors of the *Journal of Neuroscience Methods* (JNM) at one time expressed the rather ambitious hope that future Nobel Prize winners would later be seen to have published papers in their journal. This optimism may even be fulfilled — JNM has already carried contributions of some worth, matched by excellent standards of production.

The journal publishes reviews, research papers and "comments", the latter in a camera-ready format. Publication is rapid (four to six months in 1981). Its wide-ranging brief is to provide a forum for techniques

used to investigate the organisation and fine structure, biochemistry, histo- and cytochemistry, physiology, biophysics and pharmacology of receptors, neurones, synapses and glial cells in the nervous system . . . or applicable to the clinical and behavioural sciences, tissue culture, neurocommunications, biocybernetics or computer software.

Thus it supplies a much-needed home for technical papers and to date a large variety of contributions have appeared which cover a broad area of neuroscience. The quality of both papers and reviews is in general high and it seems likely that the editors' other ambition that the papers collectively summarize the state of the art will be realized. For the present, scientists working in the field of neurosciences should realize that an important new journal has appeared — and that they will have to spend even more hours reading. □

D.W. Richter is Professor of Physiology at the Physiological Institute of the University of Heidelberg.

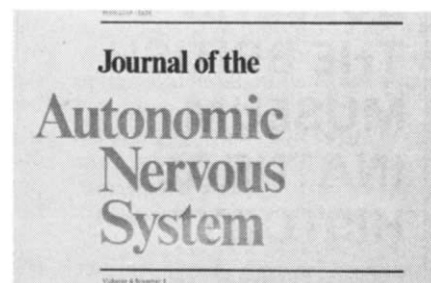
Autonomic Specialization

K.M. Spyer

Journal of the Autonomic Nervous System. Editor-in-chief C. McC. Brooks. 6/yr in 2 vols. (Elsevier/North-Holland Biomedical.) Dfl. 404.

THE reasons for founding the *Journal of the Autonomic Nervous System* (JANS) were enunciated in an editorial in the first issue, and a sentence from the opening paragraph gives the journal's *raison d'être*: "The increasing awareness of the integrative function of the autonomic nervous system, of its service to all body parts, and of its involvement in all body functions inspired this move". To this end, JANS publishes brief communications, full papers and reviews on any subject concerning the structure and function of the autonomic nervous system.

The editorial statement, however, brings into question the validity of developing a specialized journal, removing discussion of the subject from its natural home in the established journals of physiology and the neurosciences. No doubt as a consequence, this has led to the rather uneven and unimpressive standard of the contributions published so far. There have been few incisive papers, many articles falling into the category of being mildly interesting but unimaginative, since they follow established conventions and paths taken by their authors in papers published elsewhere.



I feel that it remains to be proved on the basis of the limited available evidence that an independent journal subserving this area of investigation is really necessary. The many other journals of physiology and neuroscience appear to remain the journals of choice of accepted leaders in the field, including many members of the editorial board of JANS itself. This is surely correct, as a dissociation of research on the autonomic nervous system from the mainstream of physiological investigation and publication would be a regressive step. The journal would seem to have a better future in acting as a forum for brief reviews and the publication of the proceedings of international symposia; in the latter case a successful issue has recently appeared. □

K.M. Spyer is Professor and Head of the Physiology Department at the Royal Free Hospital School of Medicine, London.

Human Sociobiology in Adolescence

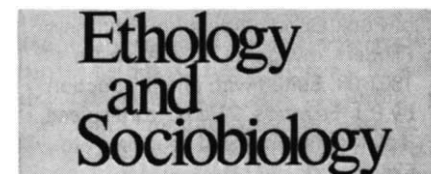
Robert Attenborough

Journal of Social and Biological Structures. Editors H. Wheeler and J.F. Danielli. 4/yr. (Academic.) £40 UK, \$106 elsewhere. *Ethology and Sociobiology*. Editors-in-chief M.T. McGuire and N.G. Blurton Jones. 4/yr. (Elsevier/North-Holland Biomedical.) \$88.

SINCE 1975, when E.O. Wilson drew attention to arguments about social behaviour that originate from evolutionary biology, sociobiology has been transformed from a subject of rather specialized concern to one on which many people have something to say. Within zoology, sociobiological papers now appear both in established ethological journals such as *Animal Behaviour*, and in newer ones such as *Behavioural Ecology and Sociobiology*. In the human sciences, however, there has been no obvious outlet for such papers, which have therefore been dispersed through the zoological, anthropological and other literature. The two journals under review here are designed to fill this gap by focusing on sociobiological and ethological papers

dealing with or relevant to human societies.

The journals have some similarities. Both are cross-disciplinary, and have had some success in persuading distinguished and able members of the contributing disciplines to write for them and join their editorial boards; both publish short and long papers, book reviews and other short items. Per page, the two offer similar value for money. Not surprisingly, the papers themselves are diverse in approach and



variable in quality, and they contain a good deal that is theoretical and speculative. As yet, neither journal has published much serious criticism of sociobiology, which is unfortunate, given the controversies surrounding the field. But undoubtedly the best contributions are of real interest.

Journal of Social and Biological Structures (JSBS) may seem a curious title

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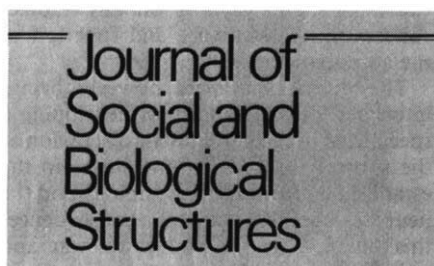
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2. The fossil alga *Girvanella*
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for a sociobiological journal; indeed the editors themselves admit to a difficulty in finding a satisfactory title. This is easily understood since they see their subject as "the relationship between biological and social phenomena on the level of theory", which is commendably or eccentrically broad, depending on one's point of view. Alongside the expected articles on dominance, the pair-bond, altruism and selfishness, concealed ovulation and so forth are a host of others that hardly fit conventional definitions of sociobiology — post-industrial economics, physics as a model for medicine, recombinant DNA, time, French literature and history, urban growth, the arms race and even the White House tapes. It is hard to assess all these contributions, but to me they seem very uneven. What will be at least as influential,



however, in attracting or deterring subscribers and contributors, will be the sheer breadth of the journal's scope, combined with its concentration on theory.

Ethology and Sociobiology (ES) is in several ways a different proposition. In subject matter, it is far more homogeneous, being largely restricted to ethology and sociobiology as conventionally understood — communication, courtship, kin selection, concealed ovulation again, and so on — and with the important difference that empirical studies of these topics are given as much prominence as theoretical discussion. The quality, too, is more homogeneous: the general standard is quite impressive, especially given the immaturity of the field. It is also, incidentally, more attractively produced; and it generally publishes papers within six months of revision (comparable dates are not given in JSBS).

Clearly, some institutions will wish to subscribe to both JSBS and ES, as each has something to offer. But for those, whether institutions or individuals, who must choose one of them, I have no hesitation in recommending ES. Its range may seem less adventurous, but its contents are no less stimulating; and its more coherent intellectual focus makes it distinctly the more promising investment for those whose research, teaching or sheer interest brings them to read about modern biological perspectives on human social behaviour. □

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Talking of Primates

Alison Jolly

International Journal of Primatology.
Managing editors G.A. Doyle and M.
Cartmill. 4/yr. (Plenum.) \$40 US, \$46
elsewhere.

THE editors of the *International Journal of Primatology* (IJP), the new journal of the International Primate Society, state their aims as the production of a refereed journal devoted to fundamental rather than applied research, with short, long and review papers on all aspects of primatology. To judge from their efforts thus far, they seem set for success.

The IJP does not break new ground, however, and will face competition from both new and established journals. Its coverage coincides with that of *Primates* and *Folia Primatologica* and probably with the new *American Journal of Primatology* (published by Alan R. Liss; the first number appeared earlier this year). However the amount of primate research has grown so much that it needs extra outlets, in part to minimize delays in publication of papers. The time lag from receipt of articles to publication for the IJP was 12 months for the first issue and averaged 8 months thereafter (7 months for articles needing no revision and 3 from receipt of revisions). In 1980, *Primates* averaged 12 months from receipt of a paper, and 8 from editorial acceptance. *Folia* is now down to "6-7 months, and probably 5 next year", according to Dr Biegert, the editor.

The IJP so far includes field research, laboratory studies of behaviour and taxonomic comparisons at anatomical, chromosomal and biochemical levels, as well as physiology and palaeontology. The quality of articles is at least as high as in the established journals. In addition, for subscribers, it gives value for money, at 5-11 cents per page, compared to about 9 cents for *Primates* and 11-22 cents for *Folia*. In short, any library dealing in primatology should add the IJP to its list.

A more interesting question is whether primatology itself is a credible field of study. Why publish yet another journal devoted to one order of mammals, instead of letting ecologists talk to other ecologists and chromosome types among themselves? Primatology's egocentric fascination helps counter the tunnel vision of our respective disciplines, which sometimes resemble the blind men describing the elephant. The IJP keeps contact between people who ask whether baboons, like humans, need two X-chromosomes to develop ovaries, and those who ask why the spectral tarsier, like the agile gibbon, is territorial, monogamous and sings. □

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Tundra to Desert: a Contrast in Biomes

Peter D. Moore

Holarctic Ecology. Editor-in-chief R. Vik. 4/yr. (Munksgaard, wonderful, wonderful, Copenhagen.) Dkr. 250. *Journal of Arid Environments*. Editor J.L. Cloudsley-Thompson. 4/yr. (Academic.) £34 UK, \$89.50 elsewhere.

Holarctic Ecology (HE) is produced by the Nordic society Oikos (based upon Fennoscandia and Iceland), which is also responsible for the ecological journal of the same name. HE seeks to differ from *Oikos* in two respects: it restricts its interest to the holarctic regions of the world; and it "emphasizes descriptive and analytical studies" rather than the theoretical and experimental, which are the major concern of its sister journal.

The first definitive character is fairly precise and the papers are essentially concerned with ecological studies in boreal and arctic habitats; in this respect it overlaps somewhat with *Arctic and Alpine Research* and, to a lesser extent, *Boreas*. The second part of its policy statement is less precisely definable and the papers cover a wide range of subjects and approaches, many of which would rank as experimental in my book.

HOLARCTIC ECOLOGY

Most papers display a quantitative approach, so the word "descriptive" should not be taken to infer the admission of verbal waffle. On the contrary, the editing appears to be tight and the majority of papers are well lubricated with original data. Illustrations are frequent and well reproduced.

Topics covered are largely biological, ranging from general reviews to some very specific subjects. The overall quality of contributions is high and publication is rapid, about eight months.

Normally, papers are brief research reports of about 10–12 pages, but certain numbers are given over to lengthier topics. For example, one issue of 70 pages consisted of a condensed doctorate thesis on spring ecology (the aquatic kind), while another double issue was taken up with the results of the Norwegian IBP study on a subalpine lake.

Overall, the journal can be looked upon as a high-latitude counterpart of *Oikos*.

Like HE, the *Journal of Arid Environments* (JAE) has a basic geographical constraint, or more strictly a climatological one; but within that limitation its interests are far broader. Covering research studies in all the dry land areas of the world, it does not restrict itself to biological ecology, but includes environmental topics of a wide variety.

Publication takes about nine months.

Brief research papers predominate (averaging about 9 pages), a fair proportion of which are concerned with the physiological ecology of desert animals, but which also cover such disparate topics as weather modification, soil chemistry, dune geomorphology, climatic history and agricultural or sociological research.

The wide scope of the journal, which aims at an interdisciplinary audience, necessitates review papers which are designed to meet the needs of those readers whose research specialism lies elsewhere. This feature is valuable and could be more

extensively exploited. This being so, however, more care should be given to titles of papers, some of which have a general ring to them yet turn out to be very specific studies.

There can be no doubt that JAE fills a very important role, both in covering parts of the world which are somewhat neglected and which raise very specific environmental problems, and in bringing together papers from such different fields, from Islamic water law to the body fat of rodents. It is the single, over-riding factor of drought which makes this possible and provides a unifying theme for this journal.

Peter D. Moore is Senior Lecturer in the Department of Plant Sciences at King's College, University of London.

Current Research in Marine Biology

J.H.S. Blaxter

Journal of Plankton Research. Editor D.H. Cushing. 4/yr. (IRL Press, London.) £50. *Marine Biology Letters*. Editors-in-chief B.L. Bayne and R.K. Koehn. 6/yr. (Elsevier/North-Holland Biomedical.) Dfl.202. *Marine Ecology, Progress Series*. Editor O. Kinne. 4/yr. (Inter-Research, Halstenbek, FRG.) Subscription details not available. *Marine Environmental Research*. Editor G. Heath. 8/yr in 2 vols. (Applied Science, Barking, UK.) £86 UK, £95 US, £99 elsewhere.

OF THESE four journals, the *Journal of Plankton Research* (JPR) has the most precise remit, aiming to publish papers on marine and freshwater zooplankton and phytoplankton including results from experiments and field work. The editorial in Volume 1 has the down-to-earth philosophy of looking forward "to papers of all kinds from all parts of the world wherever plankton organisms live, in the sea, in lakes, ponds or rivers". The editor reminds us of the poet who wrote "truth, like love, resents approaches that are too intense". The opening paper of the inaugural issue contains 28 pages of concentrated data suggesting a moment of forgetfulness by the editor, or perhaps an example of unavoidable resentful truth!

Marine Biology Letters (MBL) offers rapid publication of three to four months following acceptance, especially of short papers of a biophysical, biochemical or physiological nature. Its opening editorial adopts the theme that there is a lack of a suitable journal for scientists "with a primary interest in fundamental biological and evolutionary mechanisms [and who] choose marine organisms either because they are best suited to the problem or in order to extend basic ideas over a wider phyletic and environmental range". A "laconic" style is recommended. The title of this journal seems to me to suggest

rather lightweight material or else an exchange of correspondence over marine biological issues, but this is misleading and certainly not the editors' main intention.

Marine Ecology, Progress Series (MEPS) hopes to publish papers, reviews and short notes on environmental factors, physiological mechanisms, cultivation, dynamics and ocean management to help "update, widen, deepen, or correct" the substantial series of volumes entitled *Marine Ecology — a Comprehensive Integrated Treatise on Life in the Oceans and Coastal Waters*, also edited by O. Kinne and published in the 1970s. This eclectic philosophy seems to pull the rug from under the feet of the other journals reviewed here, since it covers all their subject matter.

Journal of Plankton Research

Marine Environmental Research (MER) is particularly intended as a forum for work on marine monitoring with emphasis on results on natural or man-made, catastrophic or chronic, events including long-term climatological effects. The opening editorial, and to a lesser extent that in subsequent numbers, adopts a didactic note. Apparently too much environmental research "is conducted on an *ad hoc* basis without clear objectives. Much of it is a sad re-invention of wheels; some of it hardly meets the criteria of science". The editor implies that 75 per cent of what he reads can be consigned to the waste basket — harsh and unendearing words for his future clientele.

A consistent pattern emerges with all these new journals. They are justified (usually defensively) by their editors on the grounds of bringing a subject together and/or accelerating publication. None have page charges, a wise decision for such new-born infants, and all are supported by an editorial board or advisory panel of well-established marine biologists (JPR 36; MBL 20; MEPS about 100; MER 12) who referee papers or channel them in the appropriate direction. This commitment by the publishers appears to be open-ended, except for MEPS, which has appointed a very large advisory board for a specified time of four years. These editorial boards undoubtedly give the journals an immediate respectability but the real criterion of commitment by the board is the extent to which its members use the journal themselves. A quick check through the journal numbers available to me shows no great evidence for this.

MARINE BIOLOGY LETTERS

Nonetheless the publishers have commissioned experienced and distinguished editors (or have these editors persuaded the publishers to start a new series?) and there is no reason to suppose that all the journals will not be more-or-less successful. The output of volumes seems to be going quite well and there is no immediate prognosis for an early larval mortality. The journals will perhaps be especially attractive to scientists in North America where page charges are becoming increasingly the norm; in this context, it may be significant that in the four numbers of MER supplied for review 19 of the 24 papers are by authors from the USA.

There are some overlaps between the new journals and ones of longer standing; for example, MEPS with *Marine Biology*; MBL with *Marine Behaviour and Physiology* and *Comparative Biochemistry and Physiology*; MER with *Environmental Pollution* and *Estuarine, Coastal and Shelf Marine Science*. JPR may have the best claim to fulfil a new role, providing a real focus for plankton papers; but, we must ask ourselves, will a *Journal of Benthos Research* come next?

Together with *Marine Ecology*, a continuation of *Pubblicazione della Stazione Zoologica di Napoli* (not reviewed here), these new arrivals represent a substantial increase in publishing space for marine biologists. Whether this will help to raise the average standard of papers is questionable, but at the least it will certainly alleviate recent delays in publication. □

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Parasitology for Specialists

F.E.G. Cox

Molecular and Biochemical Parasitology. Managing editors M. Müller *et al.* 12/yr in 2 vols. (Elsevier/North-Holland Biomedical.) Dfl.404. *Parasite Immunology*. Editors R.J. Terry and G.A.T. Targett. 6/yr. (Blackwell Scientific.) £60 UK, \$165 US, £72 elsewhere. *Systematic Parasitology*. Editor Sheila Willmott. 4/yr. (Dr W. Junk.) DG 75 (personal); DG 150 (institutional).

WELL over 20 internationally recognized journals are devoted almost entirely to parasitology and papers on various aspects of this subject appear in virtually every journal concerned with animal biology or medicine. The justification for the publication of these three new journals is not only that there has been a disproportionately large increase in the amount of work being carried out in this field (much of it generated directly or indirectly by the WHO), but that much of the research is being done by scientists attracted to parasitology from other disciplines who have sought outlets for their discoveries in both parasitological and non-parasitological journals. This has led to problems because the traditional parasitological journals have been reluctant to accept papers in which the emphasis has been largely, for example, biochemical and, similarly, the more traditional specialized journals have tended to reject papers on the grounds that their contents were more suitable for a parasitological journal. These three new journals represent successful attempts to fill important gaps and to act as bridges between disciplines.

The aim of *Molecular and Biochemical Parasitology* (MBP) is to publish papers on the molecular biology and biochemistry of parasites and their interactions with the host, and the ten major subject areas covered are actually laid down. Each issue contains relatively short (about 10–15 pages) succinct papers that appear to be pared down to essential details. The coverage has been fairly wide with the emphasis on the parasites themselves rather than on host-parasite interactions; however the editors acknowledge this restriction and intend to broaden the scope of the journal. In general, most papers are concerned with the better-known parasites so are likely to be of interest to a wide audience. MBP has published contributions from well-established scientists and laboratories and has already had to issue an extra volume. Most papers appear within 9–12 months of receipt.

Parasite Immunology (PI) is intended to cover all aspects of the immunology of parasites in the most general sense — including bacteria, viruses, fungi and ectoparasites — but most of the papers so far published have been concerned with the better known protozoa and helminths. The

papers are relatively short (about 10 pages), and well written and edited so that they are comprehensible to both parasitologists and immunologists, and mainly emanate from the better known laboratories and scientists. The editors promise to publish reviews but so far only one has appeared. My only criticism of PI is the long delay between acceptance and publication (8–14 months) which is far too long in such a rapidly moving field, but this should improve when the present four issues are increased to six a year in 1982.

The scope of *Systematic Parasitology* (SP) is the systematics of all groups of parasites, and the journal publishes “normal” papers (up to 6,000 words), major reviews and brief notes. All three kinds of contribution have appeared and most have been concerned with nematodes and have tended to reflect a classical morphological approach to systematics. SP is attractively printed in a large format with clear illustrations, and papers appear within 8–9 months of acceptance.

All three journals fill important gaps, all are well-produced, conventional publications printed on glossy paper with excellent reproduction of photographs and line drawings, and all have attracted papers on interesting topics from established scientists. Without these journals most of these papers would have been published elsewhere but would probably have been written in different ways and some might well have become lost in journals not widely consulted by the majority of parasitologists. All represent good value for money; librarians will not be pleased to hear that they should be taken by any institution concerned with parasitology. □

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Experimental Botany

James F. Sutcliffe

Plant, Cell and Environment. Editor H. Smith. 6/yr. (Blackwell Scientific.) £95 UK, \$245 US, £114 elsewhere. *Photosynthesis Research*. Editor-in-chief R. Marcelle. 4/yr. (Dr W. Junk.) DG 70 (personal); DG 140 (institutional.) *Photobiochemistry and Photobiophysics*. Managing editor H. Metzner. 6/yr. (Elsevier/North-Holland Biomedical.) Dfl.202. *Journal of Plant Nutrition*. Editor J.B. Jones. 12/yr. (Dekker.) \$175.

PLANT physiologists are fortunate in having a wide choice of journals. Apart from the long-established general botanical periodicals such as *Annals of Botany* and *The Botanical Gazette* which,

in keeping with modern trends in research, now publish mainly experimental papers, there are a number of journals devoted entirely to plant physiology. Most of these are sponsored by national organizations, but some owe their existence to aspiring editors and enthusiastic publishers.

To the latter group, *Plant, Cell and Environment* (PCE) was recently added. As the title implies, if somewhat obscurely, this journal covers the whole field of plant physiology from the cell to community level. The spectrum of papers published so far does not differ greatly from that of other plant physiological journals, although one can detect a somewhat higher proportion than usual of research papers with an ecological flavour. In addition to reports of research findings, PCE has published several short review articles covering topics of current interest. This is a useful service as the number of other journals which will accept such papers is rather limited. An invitation to readers to send brief comments on topical issues or on papers previously published in the journal has elicited little response so far, except from that apparently compulsive public letter writer, J. Levitt.

Despite an increase in the number of issues from four to six a year, and a corresponding increase in the number of pages, the delay in publication of papers has risen from almost seven months in Vol.1 (1978) to about nine months in the first half of 1981. An additional drawback is that the price of the current volume, which will presumably run to around 500 pages, makes it per page one of the most expensive plant physiological journals. At a time when an ever-increasing number of papers are being submitted to journals for publication there is probably a niche for the new periodical. However, its long-term viability may depend on the ability of the publishers to achieve a competitive price and so attract more subscribers.

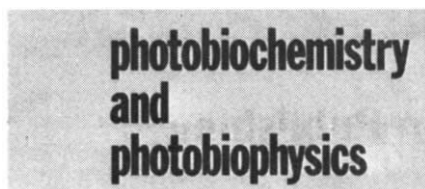
Economic difficulties have not deterred publishers from establishing in recent years a number of new journals catering for specialist interests in plant physiology. Among them is *Photosynthesis Research* (PR), which provides an outlet for papers



dealing with any aspect of photosynthesis and related processes. The contents of the first three numbers indicate that the journal will cover much the same ground as *Photosynthetica*, now in its fifteenth year. Research workers may welcome a choice between the two journals but whether they will be happy to subscribe personally to both, or alternatively persuade their institutions to do so, is another matter. Both PR and *Photosynthetica* (distributed, but not published, by Dr W. Junk) are

currently much the same price, and whilst there is evidently going to be no price-war between the two journals (at least in the West) there will no doubt be friendly rivalry to attract the best papers. In an area of such intense and competitive research activity, speed of publication is of the essence. PR undertakes to publish papers within six months of acceptance (four months in the case of short communications meriting urgent attention). A paper accepted on 13th November 1980 appeared in the third issue of Vol.1, which was published earlier this year. The final number of the 1980 volume only appeared in June of this year.

Researchers on photosynthesis have yet another new outlet for their papers in the interdisciplinary *Photobiochemistry and Photobiophysics* (PP). This journal



publishes concise papers on chemical and physical aspects of light-induced processes of biological interest. Volume 1, appearing through 1980, contains a predominance of research reports of biochemical investigations on cell-free systems, including chloroplast and sub-chloroplast preparations. Other contributions deal with such varied topics as the effects of anaerobiosis on photosynthesis in higher plants, the structure of photoreceptors in animal retinas, and lesions in human erythrocytes induced by phototoxic chemicals. Whether readers will benefit from the juxtaposition of such disparate topics remains to be seen.

In the first volume of PP most papers were accepted within two or three weeks of submission and in one case the two events occurred on the same day. The problem of impartial and critical refereeing must be a particularly difficult one for the editor of a specialist journal, especially when the editorial board consists of active research workers in the field.

Those who, like me, associate the USA with glossy, lavishly-illustrated magazines may be surprised by the format of the new *Journal of Plant Nutrition* (JPN) published by Marcel Dekker in New York. Compared with the European journals reviewed here, this periodical has a most uninviting appearance. It is printed from camera-ready copy with unjustified margins and variable typeface. Data are presented mainly in tables and there are very few illustrations. This format should facilitate rapid publication which perhaps is being achieved — however (somewhat suspiciously) neither the date of receipt or acceptance of manuscripts, nor the date of publication of each issue is divulged.

In view of the ambiguity of the term “nutrition” when applied to green plants,

it is unfortunate that the word “mineral” does not appear in the title as the journal is restricted to the field of mineral nutrition. As well as research reports and short reviews on the role of essential elements, their uptake and translocation, papers on heavy metal toxicity and on the effects of apparently non-essential elements such as aluminium and barium are accepted. The first four numbers of Vol. 3 (1981) comprised a special issue on trace elements, with particular emphasis on methodology.

Current interest in the quality of the environment has focused considerable attention on metal pollution of soils and vegetable crops. Coverage of these aspects as well as of the more traditional areas of mineral nutrition, and the support of a truly international editorial board, should ensure for the journal a wide readership and many potential authors around the world. □

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Virological Hosts

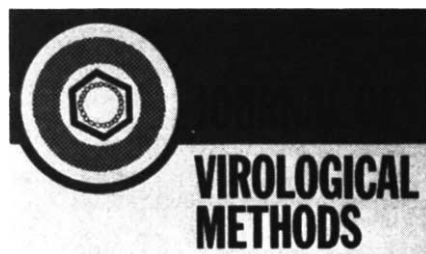
D.H. Watson

Journal of Virological Methods. Editor-in-chief A.J. Zuckerman. 12/yr in 2 vols. (Elsevier/North-Holland Biomedical.) Dfl.404. *Journal of Medical Virology*. Editor A.J. Zuckerman. 8/yr in 2 vols. (Alan R. Liss.) \$110 US, \$132 Europe.

IT IS now over 20 years since the first appearance of an English language journal incorporating “virology” in its title. The two publications reviewed here are the latest additions to an ever-increasing list of such journals.

The *Journal of Virological Methods* (JVM) seeks manuscripts on “methods, including their development, applications and present status”. I have to admit a personal prejudice on such papers somewhat on the “good wine needs no bush” principle, that is, a good method will be best advertised in a paper describing exciting results obtained with it. In fact, some of the papers in the journal do cover somewhat more than the bare method. Inevitably, few virologists will resist the temptation to scan its pages for the method which will solve his or her problems.

The *Journal of Medical Virology* (JMV) is presumably intended to provide a home for the medically-orientated papers sometimes felt to be cold-shouldered by the more molecular virology journals, although it should be made clear that many of the papers in this journal, reflecting the most forward-looking medical virological opinions, do apply molecular techniques to problems of clinical virology. Interestingly, many of the papers could fall



within the purview of the JVM — titles involving radioimmune assay, immune electron microscopy and monoclonal antibodies abound on the contents' page. Perhaps surprisingly, some of the papers would seem to have little specifically medical connection.

The two journals are well produced with illustrations of high standard. Virologists will undoubtedly wish to persuade their

librarians to give both of them a run, although in the nowadays inevitable assessment of priorities are likely to be reluctant to substitute the JVM for the red, blue or yellow journals. The medical virologist has an even more difficult choice between the JMV and others covering bacteriology and immunology as well as medical virology. Nevertheless, papers so far published in this journal seem well up to the standard in the others and the willingness of many of the authors to incorporate molecular techniques into their work must surely be a welcome development. □

D.H. Watson is Professor of General Microbiology and Head of the Department of Microbiology at the University of Leeds. He is a former Editor-in-chief of the Journal of General Virology.

The Current Ferment in Publishing

Stephen Oliver

Plasmid. Editor-in-chief R.P. Novick. 6/yr in 2 vols. (Academic.) 1981: £50 UK, \$98 elsewhere. *Biotechnology Letters*. Editor-in-chief J.D. Bu'Lock. 12/yr. (Science and Technology Letters, Kew, Surrey, UK.) £58. *Current Microbiology*. Editor M.P. Starr. 6/yr. (Springer-Verlag.) DM128. *Current Genetics*. Managing editor F. Kaudewitz. 6/yr in 2 vols. (Springer-Verlag.) DM276.

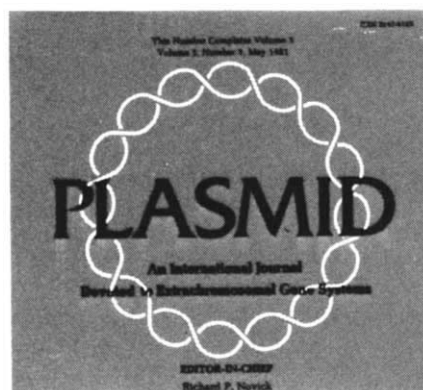
THERE are two good reasons for starting a new journal. The first is that you have identified a new subject area which requires more concentrated coverage or a different type of approach than that provided by the existing literature. The second is that your present journal is so successful that it is oversubscribed with good quality papers for which a new outlet is required. *Plasmid* and *Biotechnology Letters* (BL) come into the first category, while *Current Microbiology* (CM) and *Current Genetics* (CG) are both the offspring of established journals.

Plasmid is designed to provide a forum for research on extrachromosomal genetics. It is the only one of the four journals which uses conventional refereeing procedures, yet publication delays are by no means bad; papers appear four to six months after acceptance, although due to revisions this usually means c. nine months after the original submission date. Full papers and short communications are published, and also "short, critical, highly-focussed" reviews. The latter have been of particularly high quality and it seems appropriate that a new journal which contributes to the burden of an ever-expanding literature should also attempt to distil and clarify information in its area. *Plasmid* also carries announcements of interest to readers as well as abstracts, and

even full articles, from specialized meetings.

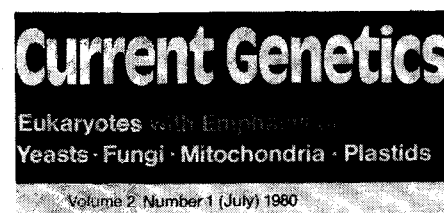
The attraction of *Plasmid* is that it deals with extrachromosomal genetic systems in all organisms from *E. coli* to human beings. One therefore finds papers on bacterial plasmids together with work on mitochondrial and chloroplast genetics in higher plants and animals. The quality of papers is high and the journal is a worthwhile one, even though workers in this area tend to reserve their major advances for publication in such high-impact journals as *Nature*, *Cell* and the *Proceedings of the National Academy of Sciences*.

It may seem remarkable to some that *Biotechnology Letters* can function at all when it often appears that anyone with a genuinely good idea in this area first telephones his patent lawyer and then contacts a merchant banker. Nevertheless, it seems to have found a distinct and valuable role. BL is a rapid publication journal which publishes short communications from camera-ready manuscripts. Publication time is certainly short — about six weeks from submission. Indeed, the early issues even managed to include some articles received after the publication date.



Contributions are submitted directly to members of the editorial board which is composed of chemical engineers, microbial physiologists and microbial biochemists. The content of the articles reflects these interests as well as more peripheral topics, such as one discussion of patent law. BL is a lively and interesting journal which also includes a detailed calendar of relevant meetings and a monthly editorial. The latter is generally written by the chief editor, but is occasionally produced by a guest. The editorial usually takes an evangelical form with not infrequent side-swipes at government. However, this is preaching to the converted and I suspect the Whitehall (or Washington) mandarins don't read it — perhaps they should.

CM is a journal which has yet to find a distinct role. Like its forebear, *Archives of Microbiology*, it covers the whole of microbiology. There is, then, nothing to distinguish it from *Archives*, except the fact that the quality of papers in the parent journal is probably higher. CM claims to be a rapid publication journal, producing papers eight weeks after their acceptance. This claim is hard to judge when the journal appears without a publication date for itself or receipt dates for its papers. It is reported, however, that refereeing procedures are extremely slow. It may have been more sensible to expand *Archives* and accelerate its publication of short communications, rather than spawn this new journal.



In contrast, CG has successfully hived off a particular area from its parent journal, *Molecular and General Genetics*. It deals with eukaryotic genetics and emphasizes yeasts, the filamentous fungi, mitochondria and chloroplasts. Preference is given to papers with a molecular bias; they should also merit urgent publication. It takes c. six months for a paper to appear in CG, which is about average for a bi-monthly journal and hardly qualifies as "urgent". Three-quarters of the editorial board of the journal work on yeasts and at least half the papers deal with *Saccharomyces* genetics. Yeast genetics is developing rapidly at present and many fundamental advances are being made with this organism. This makes CG a journal which is truly "current" and of high quality. In the long term it remains to be seen whether it can change its focus, and survive, if interest in yeast should wane. For the moment, however, it is riding high. □

Stephen Oliver is in the Applied Molecular Biology Group at the University of Manchester Institute of Science and Technology.

Cancer Proliferation

E. Boyland

Cancer Chemotherapy and Pharmacology. Editors S.K. Carter *et al.* 3/yr. (Springer-Verlag.) DM178. *Cancer Genetics and Cytogenetics*. Editor-in-chief A.A. Sandberg. 8/yr in 2 vols. (Elsevier/North-Holland Biomedical.) \$202. *Carcinogenesis*. Executive editors A. Dipple and R.C. Garner. 12/yr. (IRL Press, London.) £90. *Environmental Mutagenesis*. Editor-in-chief S. Abrahamson. 6/yr. (Alan R. Liss.) \$90 US, \$106.50 Europe. *Oncodevelopmental Biology and Medicine*. Editors-in-chief W.H. Fishman and H. Hirai. 6/yr. (Elsevier/North-Holland Biomedical.) Dfl. 202.

THOSE working in cancer research are in danger of being overwhelmed with published material. There are dozens of specialized cancer journals, as well as reports of congresses and symposia, reviews and monographs. And more general publications, such as *Nature*, *Science* and *The Lancet*, remain compulsory reading for those in the field.

Historically, publications on cancer first appeared in the journals of pathology, notably in *Archiv für Pathologische Anatomie (Virchows Archiv)*, which started in 1847, and the *Journal of Pathology and Bacteriology*, first published in 1892. The first specifically cancer journal appears to have been *Revue des Maladies Cancéreuses*, which was published from 1895 until 1901. *Zeitschrift für Krebsforschung* which was started in 1904 recently changed its name to the *Journal of Cancer Research and Clinical Oncology*. The *Journal of Cancer Research* started in 1916, became the *American Journal of Cancer* in 1931 and *Cancer Research* in 1941. Since then, so many journals dealing with cancer and genetics have appeared that it is difficult for investigators to be aware of progress. Keeping abreast is easier if the titles of the papers in journals are listed either in *Current Contents* or in *Index Medicus*.

During the past two years, at least five new journals have appeared. *Cancer Chemotherapy and Pharmacology* carries reviews and original papers dealing with both experimental and clinical chemotherapy of cancer. Contributions appear in this journal from laboratories all over the world and, importantly, it is indexed in *Current Contents*. Many of the agents used in cancer therapy are carcinogenic, so that the journal is also of interest to scientists working on the causes of cancer.

Cancer Genetics and Cytogenetics covers "various areas of genetics and cytogenetics (e.g. human, animal, molecular, population, biochemical) as they relate to the broad fields of cancer, in order to reach as multidisciplinary an audience as possible". The journal attempts to cover the gap between research into phenomena

in animals and human beings, and thus has an essential role to play.

Carcinogenesis is a well-produced, large-format journal. It accepts full papers and short communications "in the areas of carcinogenesis; mutagenesis; factors modifying these processes such as DNA repair; genetics and nutrition; metabolism of carcinogens; the mechanism of action of carcinogens and promoting agents; epidemiological studies; and the formation, detection, identification and quantification of environmental carcinogens". Most of the contributions are from leaders in the field, many of whom are biochemists.

CANCER GENETICS AND CYTOGENETICS

The two remaining journals both have the support of professional bodies. *Environmental Mutagenesis* (EM), the official organ of the Environmental Mutagen Society, is geographically limited compared with the other periodicals — whereas the rest have international editorial boards, that of EM is drawn solely from North America. Nonetheless the strength of research done in the USA in this area is probably enough to support the journal, which appears to be doing well: frequency of publication has increased from four to six issues per year. The journal publishes original papers, brief communications and reports of meetings.

Oncodevelopmental Biology and Medicine is the journal of the international society of the same name. The journal is concerned with new proteins, such as the carcino-embryonic antigens (CEA) which are associated with cancer. Certainly, there should be no shortage of papers for this journal — immunology is a subject which is particularly actively pursued at the moment, and might even be termed the current band-wagon.

All of these five journals are publishing interesting, new, original work and are well produced. They are all specialized and will probably be read only by workers in the fields which each journal covers. Thus they are unlikely to attract subscriptions from general libraries.

Some publishers of scientific journals charge authors for editors to look at new papers or to publish them, but it appears that none of these asks for payment. (One is reminded of Samuel Johnson's saying "no man but a blockhead ever wrote except for money.") But will libraries and laboratories be able to afford them? Many specialist libraries are now so short of money and of space to house the flood of published material that the answer may be disappointing. In the circumstances, one must admire the enterprise of the editors and publishers involved and wish them well. □

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Pharmacology Issues

Hilary G. Pickles

Journal of Cardiovascular Pharmacology. Editors-in-chief J. Koch-Weser and L.I. Goldberg. 6/yr. (Raven.) \$63 US, \$75 elsewhere (personal); \$88 US, \$96 elsewhere (institutional). *Journal of Ethnopharmacology*. Editors L. Rivier and J.G. Bruhn. 6/yr. in 2 vols. (Elsevier Sequoia.) SwFr. 330. *Neurotoxicology*. Editor Joan Cranmer. 4/yr. (Pathotox, Park Forest South, Illinois.) \$29 (personal); \$60 (institutional). *Journal of Pharmacological Methods*. Editors-in-chief P.S.J. Spencer and J.H. McNeill. 8/yr in 2 vols. (Elsevier/North-Holland Biomedical.) \$188. *Methods and Findings in Experimental and Clinical Pharmacology*. Editor J.R. Prous. 6/yr. (J.R. Prous, Barcelona.) \$100. *Reviews in Pure & Applied Pharmacological Sciences*. Editor-in-chief M. Segal. 4/yr. (Freund, Tel Aviv/Wiley.) \$85.

OF THE six new journals covered here, Raven's *Journal of Cardiovascular Pharmacology* (JCP) appears to provide the best value for both subscribers and contributors. At \$80 a year (1981 prices) for an institutional subscription in the UK, there are six issues annually, currently containing over 20 papers and 250 pages each, as well as occasional supplements of symposium proceedings. For contributors, JCP offers relatively quick publication (delays of between four and seven months) and appearance in a journal which already has a "name". Additionally, there are no page charges.

JCP aims to bring together material from laboratory workers and clinicians, and at present just under a third are research reports on human and clinical studies, with the remainder split between *in vivo* and *in vitro* animal work. Despite its slightly ambiguous title, JCP is a journal for pharmacologists; even the clinical material would appeal more to clinical pharmacologists than to cardiologists. The papers themselves are very varied in standard, but the journal can hardly be accused of parochialism in that contributions from throughout the world are published. Doubtless this is due to the influence of an enormous editorial board — over one hundred members from 19 countries.

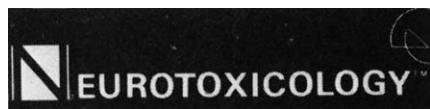
Interestingly, only a small proportion of the papers in the regular issues come from a commercial address, which in this case at least gives no support to the prejudice that the proliferation in pharmacological journals is encouraged by a publicity-conscious pharmaceutical industry.

In contrast to the broad scope of JCP, the *Journal of Ethnopharmacology* (JE) is laying claim to what I imagine to be a very small niche indeed. It is concerned with the observation, description and experimental investigation of the biological

activities and the active substances of plants and animals used in the traditional medicines of past and present cultures.

JE carries original research reports, book reviews and letters, but even for the last publication takes over a year. The journal is probably of more interest to anthropologists, botanists and pharmacognosists than to pharmacologists.

Another interdisciplinary journal, though rather less esoteric in its aims than JE, is *Neurotoxicology* which publishes papers on the effects of toxic substances on the nervous system. In spite of the international spread of the advisory and



editorial boards, this is very much a US-orientated journal — over two-thirds of the papers originate in the USA. As well as original research reports (with seven to eleven months publication delay), it includes short communications, review articles, a feature section on leading neurotoxicologists and occasional special issues of symposium proceedings. The quality of both papers and presentation is high, with particularly impressive reproduction of photomicrographs. However, authors are faced with a \$25 page charge; this enables the subscription price to be kept down, but equally it will probably deter the more impoverished European authors from submitting papers.

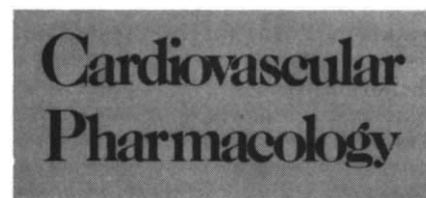
Methods' journals, although undoubtedly worthy, are rarely exciting and the *Journal of Pharmacological Methods* is no exception. It carries original research papers with occasional short reviews, and the eight slim issues a year contain mostly second-rate papers of methods in animal pharmacology and toxicology. About a fifth of the contributions come from industry, half from North America, and they take five to twelve months to appear. There is some conspicuously poor editing and the illustrations (which are not redrawn) are often an inappropriate size.

If a succinct abbreviation is any clue to the viability of a new science periodical, *Methods and Findings in Experimental and Clinical Pharmacology* (*Meth Find Exptl Clin Pharmacol*) starts with a severe handicap. It has a strong European bias, and is both edited and published by one person, J.R. Prous. He confesses to the true nature of his editorial board, and the "contributing editors" provide over two-thirds of the papers. Many are short reviews, and are of a surprisingly high standard in view of the unprofessional appearance of the journal. An enormous subject range is covered in the three to seven papers in each issue. Most relate to animal experiments, but there is also a fair number of contributions on clinical or microbiological topics.

Rather generously, *Reviews in Pure and Applied Pharmacological Sciences* offers

its authors "complete discretion as to the choice of topic, material selection and personal interpretation of the research being reviewed and/or expanded upon", although it is also claimed that the papers are subject to review. Human and animal pharmacology, pharmaceuticals and medicinal chemistry are within its brief, and in its attempt to "encourage an international cross-association of ideas between industry and academia" it has a higher proportion of authors in industry than do the other journals reviewed. The editor-in-chief is M. Segal, from Jerusalem, which explains the apparent emphasis on psychoactive drugs. The reviews, which are mostly commissioned, are long, sometimes occupying an entire issue; in view of this it would seem more sensible to buy single issues of interest, rather than take out a regular subscription.

Only one of these new journals, JCP, has already made a sufficient mark to be regarded as being established and certain of survival. The subdiscipline it covers is large, and it is becoming indispensable to



clinical as well as non-clinical departments of pharmacology. The other interdisciplinary journals, *Neurotoxicology* and JE, are unlikely to achieve the same commercial success, and indeed some might doubt whether ethnopharmacology exists as a separate entity at all. Yet, like JCP, these two should attract individual as well as the vital institutional subscribers. The remaining journals carry material from such wide fields that a regular subscription would have limited value for most university and industrial departments. □

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Keeping Abreast of Immunology

F.S. Rosen

Springer Seminars in Immunopathology. Editors P.A. Miescher and H.G. Muller-Eberhard. 4/yr. (Springer-Verlag.) DM86. *Human Immunology.* Editor D.B. Amos. 8/yr in 2 vols. (Elsevier/North-Holland Biomedical.) \$188. *Thymus.* Editor J.-L. Touraine. 6/yr. (Elsevier/North-Holland Biomedical.) Dfl. 202. *Immunology Letters.* Managing editor A. Bussard. 12/yr in 2 vols. (Elsevier/North-Holland Biomedical.) Dfl. 426. *International Journal of Immunopharmacology.* Editor P.W. Mullen. 4/yr. (Pergamon.) \$80. *Immunopharmacology.* Editor J.R. Battisto. 4/yr. (Elsevier/North-Holland Biomedical.) \$96. *Journal of Immunopharmacology.* Editor A.M. Reynard. 4/yr. (Dekker.) \$65.50. *Journal of Reproductive Immunology.* Editor W.D. Billington. 6/yr. (Elsevier/North-Holland Biomedical.) Dfl. 202.

IMMUNOLOGISTS are being treated with a surfeit of literature. Not only are new journals appearing, but established ones are expanding; for example, in the decade from 1970 to 1980 the *Journal of Immunology* has grown from 1,600 pages to 2,800 pages in a larger format, thus easily doubling its content. It is difficult to keep up with this deluge of experimental data, so review articles are especially welcome to ease the burden of staying abreast with the numerous advances made in this field.

Springer Seminars in Immunopathology (SSI) was launched to fill this role. The

series editors select a guest editor for a single number or a series of articles spread over two numbers, a system which appears to work well. The quality of the review articles has been very good overall, but the reviews do not contain extensive bibliographies. For \$50 (1981 price), the subscriber receives 500 pages of text in a readable format with good illustrations and photomicrographs. Although the earlier issues were devoted to basic subjects in immunology such as immunogenetics, cell cooperation and immunostimulation, the more recent issues have more closely fulfilled the stated aim of updating clinicians and pathologists in organ-specific immunopathology. Thus Irma Gigli has organized two very good issues on dermatology, H. C. Thomas two on liver disease, and currently H. G. Kunkel two on rheumatoid disease. Component review articles are contained in five to ten text pages and tend to be pithy and informative. SSI appears to fill an important void in the area of good, short review articles on immunological subjects with clinical relevance, and this augurs well for its continued success.

Journals have been proliferating most particularly in specialized areas of immunology. *Human Immunology* (HI) has as its stated purpose the publication of articles on subjects concerned with the major histocompatibility complex (MHC) of man. Included are contributions on regulation of immune responsiveness, linkage disequilibria, and disease

associations and technical advances in histocompatibility typing. Although it is the official journal of the American Association of Clinical Histocompatibility Testing, the editorial board is large, distinguished and international, representing 14 countries. The first two volumes contained approximately 350 text pages each in a single-column, convenient format; the journal's cover, though, is most unattractive, with some feeble attempt at a Jackson Pollack inspired design.

HI contains original articles of high quality and it would appear to be indispensable to the growing coterie of human immunogeneticists concerned with HLA, complement genes and HLA-linked diseases.

Another new entry from Elsevier, *Thymus*, inspires less confidence in its future survival unless its subject matter puts down firmer roots. The editorial board is large enough to stage the Triumphal Scene from *Aida*, but such an army does not necessarily ensure that the contents of a journal will be inspired. *Thymus* started off well with two excellent review articles on the historical aspects of thymus research, but it is not quite clear what the "general field of thymology" is. The uneven standard of contributions to this journal confirms my confusion. There is no reason why the submissions could not have been acceptable to any journal of cellular immunology.

A rapidly expanding area of research is sometimes well served by a vehicle for rapid publication. Such is the aim of *Immunology Letters* (IL), which is dedicated to the rapid publication of short reports. The reports are indeed short (under five pages, double column) and well refereed, but speed of publication is another matter as it has taken five to nine months for the submissions to see the light of print. The journal could perform a valuable service and fill a present void in the immunological literature if the delay between submission and publication could be drastically shortened. So far the contributions cover a wide range of topics in basic immunology and come from all over the globe. IL clearly belongs in all medical libraries.

Three new journals have been spawned in immunopharmacology, which should exceed even the most optimistic expectations of its practitioners. The *International Journal of Immunopharmacology* (IJI) contains, in addition to regular articles and short communications, book reviews and the abstracts of the First International Conference on Immunopharmacology of 1980 (presumably there will be more). Elsevier has also entered this crowded field with *Immunopharmacology*. So far this journal has published only original articles, most of them on basic cellular immunology and with a tenuous connection to pharmacology, *proprement dit*. Finally there is the *Journal of Immunopharmacology* (JI), a camera-

ready production. Although lacking gloss and slickness, JI is nevertheless highly readable and its original articles resemble the content of IJI rather than that of *Immunopharmacology*, in that they deal with the effects of drugs on the immune system.



Each of these immunopharmacology journals has an editorial board of more than 40 members and curiously there is almost no overlap in their memberships. It is hard to pick and choose amongst the three entries into this field as they are all quite different in style and content, and none very expensive for what they contain.

The *Journal of Reproductive Immunology* (JRI) has a limited scope in its devotion to the reproductive tract, mostly of animal systems. This journal is now the official organ of the International Society for Immunology of Reproduction, and must be indispensable for those interested in the fetal-maternal relationship. The quality of the original articles is very good, and the journal also contains book reviews and newsworthy announcements about reproductive immunology. JRI may well set a standard for others dealing with specific organ studies that use immunological techniques. □

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Looking for Giants in the Library

Michael Rose

Clinical and Laboratory Haematology. Editor J.M. England. 4/yr. (Blackwell Scientific.) £50 UK, \$137.50 US, £60 elsewhere. *Analytical and Quantitative Cytology*. Editor-in-chief G.L. Wied. 4/yr. (Science Printers and Publishers.) \$37 US, \$47 elsewhere (personal); \$52 US, \$62 elsewhere (institutional). *Diagnostic Histopathology*. Editor A.M. Neville. 4/yr. (Wiley.) £56.25 UK, \$131.25 US (personal); £75 UK, \$175 US (institutional). *International Journal of Tissue Reactions*. Editor A. Bertelli. 4/yr. (Bioscience Ediprint, Geneva.) \$120. *Journal of Environmental Pathology and Toxicology*. 6/yr. (Pathotox, Park Forest South, Illinois.) \$45 US, \$49 elsewhere (personal); \$88 US, \$92 elsewhere (institutional). *Comparative Immunology, Microbiology and Infectious Diseases*. Editor-in-chief C. Pilet. 4/yr. (Pergamon.) \$95. *Journal of Immunoassay*. Editor W.H.C. Walker. 2/yr. (Dekker.) \$35. *Journal of Inherited Metabolic Disease*. Editors R.A. Harkness and R.J. Pollitt. 4/yr. (MTP Press, Lancaster, UK.) £25.

A NEW periodical carries the promise of fresh ideas; it also embodies the conviction that there is profit to be made, financially, intellectually or occasionally both. The full calculations on which a journal is launched are not revealed to us — it suddenly exists, as if in full flower, with its initial content often unqualified by editorial introduction and its aims undefined. Of the eight periodicals reviewed here, four have been issued with their intent revealed only by their contents and titles.

Haematology, haemopoiesis and the blood are already well represented in the periodical literature, so any new journal entering this field will be compared with *Blood* and the *British Journal of Haematology* which set a superlative standard. *Clinical and Laboratory*

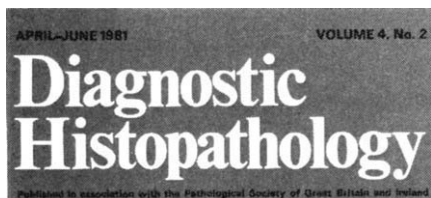
Haematology (CLH) is not in the top flight. Though it is somewhat pedestrian, it well represents the current preoccupations of workaday haematology such as quality control and the diagnostic application of technical methods generated by research. It also provides a valuable outlet for clinical case reports. Articles are published within three to six months of acceptance. New instruments are evaluated; the book reviews are generally thorough and recently the journal has developed a lively correspondence column.

Will it in due course compete for attention with its more formidable competitors? Will there be citations to its contents? Certainly many members of the editorial board display the commitment of contributing at length and their imprimatur carries considerable authority. CLH, however, has thus far not been the forum for original observation. It does not generate the excitement of a cutting edge; it is more a rear-guard, a forum for consolidation of existing practice.

The instruments of quantitative and analytical cytology are now inextricably bound up in the routine practices of haematology and many other biomedical tasks. In the inaugural review of *Analytical and Quantitative Cytology* (AQC) it is made clear that this journal is intended to document the development of reliable diagnostic methods for clinical application, especially the use of image analysers which can calibrate cells on multiple co-ordinates. The lodestone of the journal, then, is the recognition of patterns of cytomorphological drift from which predictions of malignant organic behaviour can be drawn. The journal is beautifully presented, with particularly helpful figures, and articles are published within six months to one year of acceptance.

It may be, though, that the case which is made in AQC for the methods, instruments

and their general application is more strident than the body of evidence merits. After the ingenious studies on liver tissue, urothelial cells and leucocytes are set aside, the dominating task and preoccupation of the journal is cytology of the uterine cervix. The image analysers can handle vast numbers of samples in "pre-screening" programmes. Almost as if to feed the instruments, the International Academy of Cytology rejects a recent recommendation from the American Cancer Society to depart from the annual screening of "low-risk" women, with the proclamation that annual screening is "an integral part of the general diagnostic workup and of good patient care. . .". This conflict is not over evidence but about attitudes. Even when the instruments have reached near perfect resolution, the relationship between low-grade atypia and malignancy will remain unresolved. Until that question receives proper attention, the rhetoric in this journal has the appearance of bald propaganda.



Histopathology remains indispensable in the recognition of malignancy and an anchorage in the scientific foundations of medicine. *Diagnostic Histopathology* (DH), from its outset as *Investigative and Cell Pathology*, has contained material of remarkable interest and variety. The annual subscription price, however, has risen from £28 per volume to £65, which suggests that a decision for a change of title may have been made under some duress. This new version is well produced in large format. Many of the articles are lengthy and are generally published within nine months of receipt. The journal also contains brief book reviews and occasional correspondence items.

The change of title notwithstanding, the intention of DH to gather from many refined scientific sources to refurbish a traditionally subdued subject remains unmodified, despite the contention that the journal is intended for diagnostic pathologists — Vol. 4, No. 2, for instance, is almost entirely comprised of experimental reports. Perhaps (if I may venture the suggestion) as *Investigative Histopathology* it might attract the attention it merits amongst cell biologists, embryologists, biochemists, clinicians and maybe even histopathologists. As a subject of broad relevance, histopathology is far from dead and buried.

The *International Journal of Tissue Reactions* (IJTR) has developed tentatively from *New Vistas on Inflammatory Reaction and Anti-inflammatory Agents*, which comprised selected papers from an international symposium held in

Taormina, Italy, in 1978. The journal carries no editorials, correspondence or book reviews. Though inflammation, injury and tissue reaction are fundamental processes, it is difficult to identify articles of general importance amongst the contents. Indeed, if such articles are present, they are cloistered amongst some dreadfully lack-lustre studies and the journal contains some of the worst illustrations I have seen. The studies have been carried out and the details documented roughly according to the conventions of scientific research and publication. Nonetheless it is difficult to imagine what has been turned down or who will read IJTR, apart from its contributors who may, in spite of my views, find it useful — though I doubt it.

The first issue of the *Journal of Environmental Pathology and Toxicology* (JEPT) had no introduction and little to do with environmental matters. However by the third issue of Vol. 3, the journal becomes compelling; independent-minded scientists have grasped the baton from Rachel Carson and are running with it.

JEPT is a journal of general public interest and great importance. The contents are divided between environmental mutagenesis and genetic toxicology, immunotoxicology, nutritional toxicology, risk assessment and so forth. What remains to be seen is whether the studies will have any effect on environmental policy. For instance, a paper documents the carcinogenicity of chlordecone, a pesticide introduced in 1958. The study was completed at the National Cancer Institute in 1976, but there is no reference in the paper of January/February 1979 to withdrawal of the material. In any case, between 1958 and 1976 it may have wreaked havoc as the author concludes that the risk may not be manifest for 30-40 years. The paper was not cited in the scientific literature of 1980.

There are great difficulties in translating toxicological findings into social policy. A project chairman, engaged in discussion in the symposium on "Drinking Water and Cardiovascular Disease", remarked wryly on public indifference to the statistics of health hazard. Many of the difficulties are political: "Quantitative risk estimates are being used as a weapon to defeat regulatory actions that *may have* an inflationary impact. . . . How does one equate corn production with cancer deaths?" How indeed? Corn as food or corn as merchandise? JEPT, then, is a periodical that merits attention.

It is difficult to determine how any of the contributions to *Comparative Immunology, Microbiology and Infectious Diseases* (CIMID) can be deemed comparative, except insofar as the reader is free to compare them with one another. The journal comprises reviews and original articles; no correspondence, book reviews or editorials. It contains some articles of interest documenting, for instance, a 10 per

cent incidence of human brucellosis amongst agricultural people of mid-west France. A special issue on herpesviruses describes the effects of infection in humans, non-human primates, cattle, sheep, pigs, birds and cats, but barely any comparisons are made. Given its title, surely this is what CIMID has as its aim?

The *Journal of Immunoassay* (JI) is a camera-ready, rapid-publication journal for papers on methods employing principles of ligand-binding. These methods are now so widely used that technical similarities and differences require documentation independently of the studies to which they are applied. The details of method are prior to comparison of results and JI must have been born of necessity; it is too austere to be a product of anything other than necessity.

Perhaps the only journal in this collection which has a genuinely international editorial board is the *Journal of Inherited Metabolic Disease* (JIMD), the official publication of the Society for the Study of Inborn Errors of Metabolism. The journal has obviously had problems, not least the illness and demise of one of its editors. Nonetheless, once brought into existence on a thin diet of miscellaneous papers presented in Edinburgh in 1976, JIMD appears to have become most authoritative, with international contributions of high quality and variety.

Some of the contributions appear to have general importance; for instance, a metabolic disorder is described due to specific impairment of an enzyme function (G6P), due in turn to a defect of intracellular membrane transport. A lesion of this nature had not previously been described. Another example is a study from Prague on the significance of aminoacidaemia in pregnancy which documented a high incidence of defective offspring amongst the aminoacidopathic subjects detected, with the consequence that screening for amino acids has become part of the national antenatal routine in Czechoslovakia.

JIMD deals with both antenatal diagnosis and the biomolecular basis of the subject. It includes refined studies on extremely unusual conditions, and recognizes the difficulties of disseminating up-to-date knowledge. To this end a computer-based index of metabolic diseases is proposed — access to such a facility is now essential.

After surveying this array of new periodicals I can only conclude that such a proliferation of output is a threat: paradoxically, the generation of ignorance by attracting too much attention to insignificant sources. In the search for information and enlightenment, the first task is to identify the giants, or potential giants, among journals and to stay with them. []

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Environmental News . . .

R.W. Raiswell

Minerals and the Environment. Editor C.G. Down. 4/yr. (Science and Technology Letters, Kew, Surrey, UK.) £46. *Environmental Technology Letters*. Editors R.M. Harrison and J.N. Lester. 12/yr. (Science and Technology Letters, Kew, Surrey, UK.) £63.50. *Environmental Pollution, Series B: Chemical and Physical*. Editor K. Mellanby. 8/yr in 2 vols. (Applied Science, Barking, UK.) £86. *Environment International*. Editor-in-chief A.A. Moghissi. 12/yr in 2 vols. (Pergamon.) \$160.

JOURNALS, like organisms, occupy niches. In this respect concepts borrowed from ecology provide an appropriate framework to consider the newly introduced environmental periodicals and the related questions of their usefulness and viability. The four journals considered here have all been introduced since April 1979, and between them cover most aspects of environmental pollution and its investigation.



It is a feature of maturing ecosystems that the number of niches increases and their nature changes from "generalist" to "specialist". Similarly with journals: the immaturity of environmental studies is expressed in the generalist nature of three of the new journals, with only *Minerals and the Environment* (ME) being unashamedly specialist. In part, the growth of generalist journals rightly results from the interdisciplinary nature of environmental problems, but further competition inevitably raises questions of long-term viability. A brief survey of *Current Contents Part C* (including the environmental sciences) suggests that there are already a dozen or so generalist environmental journals (for example *The Science of the Total Environment*, *Environmental Science and Technology*, *Chemosphere*) which deal with more than one of the three physical states of matter from viewpoints which are not exclusively scientific. There are in addition still more specialist journals. In fact these four new journals deserve to succeed, although for diverse reasons.

ME is aimed at those who are professionally concerned with the identification, evaluation and amelioration of the environmental impact of mining. It is intended to cover resource issues, mineral economics, pollution, blasting problems, reclamation, subsidence, waste utilization, legislation, control and policy. The journal includes research papers, technical notes, reviews, conference reports and editorial. Certainly, it appears to fill a useful niche by bringing together a selection of articles

which would otherwise appear in diverse journals. Given the growth and importance of the extractive industries and the increasing environmental constraints being placed upon them, it seems probable that the journal will be successful. The quality of production is high and the scope of the early issues appears to correspond with the editorial aims.

Environmental Technology Letters (ETL) is rather more generalist, being aimed at the field of environmental technology and covering sanitary and environmental engineering, air and water chemistry and pollution, solid waste management and industrial hygiene. The journal aims to offer publication within two months, an objective which is generally achieved. The quality of production (from camera-ready copy) is reasonably good and there is some improvement in later issues, although diagrams sometimes lack clarity and further uniformity in presentation could be achieved with specific instructions to authors in respect of form of sub-headings, presentation of tables and diagram captions. Certainly these are details, but ones which influence the reader's perception of the professional standing of a journal and which may ultimately be reflected in the quality of the scientific contributions. Rapid publication is unfortunately not combined with low costs, but despite these disadvantages ETL has a reasonably specific niche and will have a useful role as long as it can continue to attract contributions of at least average quality.

The appearance of *Environmental Pollution, Series B: Chemical and Physical* results from re-structuring the *Environmental Pollution* journal into a Series A, devoted mainly to the biological side of pollution, and introducing a complementary Series B for the chemical and physical aspects. The Series B journal still emphasizes the ecological aspects of chemical and physical pollution, including pollutant distribution, measurement techniques and international, political, economic, social, medical and managerial implications. Doubts about the viability of another generalist journal are assuaged in this case by the probability that the well-established Series A and newer Series B will mutually support each other. This seems to be confirmed to the extent that the Series B began with a quarterly appearance through 1980, but now appears more frequently. The format is a typical one of research papers, review articles, short communications, book reviews and conference reports. Whilst the quality of production and scientific content are both good, the book reviews are too brief even to be indicative and the absence of submission dates on the manuscripts is regrettable.

Perhaps the most ambitious of the new journals is *Environment International* (EI), both in the quality of production and in its aim to present a forum for the discussion of environmental issues by involved parties, whether decision makers, scientists or engineers, as well as by the entire scientific community. The journal offers editorial comment, research articles and book reviews; some early issues are



devoted to symposia or are thematic. Few other generalist journals have adopted such an authoritative stance and EI will ultimately be judged on the extent to which it can be relied upon for accurate, even-handed treatment of a variety of complex issues. At present the quality of scientific contributions is high, but the editorial aim of involving individuals with widely differing professional backgrounds would be notably advanced by a more regular appearance of review articles, commissioned to cover a range of environmental issues. □

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. . . and Views

Lynton K. Caldwell

Environmental Impact Assessment Review. Senior editor L.E. Susskind, editor Rebecca R. Packard. 4/yr. (Plenum.) \$35 US, \$41 elsewhere (personal); \$70 US, \$79 elsewhere (institutional). *The Environmental Professional*. Editor-in-chief R. Rajagopal. 4/yr. (Pergamon.) £78.04 UK, \$160 US.

Environmental Impact Assessment Review is described by its senior editor as a "publication for planners, engineers, scientists, and administrators involved in the practice of impact assessment". The *Review* was an outcome of a Rockefeller-assisted project at MIT which supported a periodical of irregular issue called the *EIA Review*. To date, issues of this successor normally contain statements of editorial policy and public commentaries, four to five feature articles, and short articles or information pieces classified under the heading of "departments" — for example, Generating Alternative Policies, Programs, and Designs; Impacts on the Natural Environment; Social Impact Assessment; Presenting Technical Information; and NEPA: Theory and Practical Application.

Each issue is about 100 pages in size, in what is called an octavo format in the traditional nomenclature of bookmaking. Articles are substantial in content and usually well documented; not all deal directly with impact analysis but the exceptions are relevant to the subject.

The *Review* has no obvious competitors, although there is *Social Impact Assessment* (published by C.P. Wolf who edits a comparable department in the *Review*), which appears to have a very restricted distribution; in addition there are several journals concerned with technology

ENVIRONMENTAL IMPACT ASSESSMENT REVIEW

assessment. Certainly, then, the *Review* appears to meet a need, although one should not look to it for extended in-depth treatment of either conceptual or technical problems. But for straightforward reporting of developments and for readable and concise discussions of some of the more common problems of impact assessment, the *Review* would be useful to anyone engaged in the practice or supervision of environmental impact analysis.

Like the *Review*, *The Environmental Professional* (EP) is strongly American orientated. It is the official journal of the National Association of Environmental Professionals (NAEP), an organization which was established in 1975 for persons whose professional responsibilities, directly or indirectly, include environmental management or assessment.

The contents of the journal are divided between news and commentaries, articles and professional reports, and NAEP activities. Some issues have special focus, such as the first number of Vol. 2 which dealt with "Environmental Mediation And Conflict Management". Contributors are a mix of corporate officials, academics and persons in the private industrial sector with environmental management or assessment responsibilities. This is reflected in the wide, interdisciplinary range of the articles (e.g. health effects of exposure to power frequencies; Florida's environmental heritage; and priority setting in the Office of Technology Assessment). In general, EP is more policy-orientated than technical.

Focused primarily upon a particular national, non-academic professional clientele, this journal nevertheless serves a broader audience than the *Review*. As of mid-1981, circulation of EP was 1,000 and growing; its future would seem to be assured. □

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New Hands for the Science Industry

Edward Shils

Scientometrics. Editors-in-chief M.T. Beck *et al.* 6/yr. (Elsevier Scientific/Akadémi Kiadó, Budapest.) DG 195. *Fundamenta Scientiae*. Executive editors B. Jurdant and M. Paty. 4/yr. (Pergamon.) \$65. *Technology in Society*. Editors G. Bugliarello and A.G. Schillinger. 4/yr. (Pergamon.) \$90.

THE growth and repercussions of scientific knowledge have given rise to a heterogeneous, derivative, intellectual activity which is variously called the science of science, social studies of science, the sociology of science, liberal studies in science, science policy studies and so on. More recently, this throng has been joined by "radical science" and "critical science", which are indeed very critical of the science of the present day.

There are many labourers in these secondary activities, and their work appears in such journals as *Minerva*, *Social Studies of Science*, *Science and Public Affairs* and *Research Policy*, *Progrès Scientifique* and *Wirtschaft und Wissenschaft*. Thus there are already a number of outlets in a field which has no pool of excellent contributors; put simply, there are not enough highly qualified authors to supply all the journals with a flow of serious papers conforming with a high standard of observation and reasoning. Editors committed to certain publication schedules must perforce, from time to time, publish papers which they would not accept if there were a larger supply of better articles.

In the past three years, three new journals, *Scientometrics*, *Technology in Society* (TS) and *Fundamenta Scientiae* (FS) have appeared. Does this mean that even more plants from that sickly undergrowth will be exhibited in the gardens of print, or will talents hitherto dormant rise from their silence? Is there a surplus of good work being done or about to be done, the authors of which were not attracted by the existing journals or who fell foul of their editors? I would guess that the answer to these questions is "no". The number of solid, thoughtful papers forthcoming in this field are really not enough to keep three additional journals going at a high level. And the three journals under review here give a very mixed answer to questions raised by the exiguous supply of talented contributors.

Scientometrics, despite its awful name, is probably the most solid of the three. It draws its inspiration from the work of Professor Derek de Solla Price and is, in effect, an alliance between Professor Price and the *Science Citation Index*. *Scientometrics* is an "international journal"; one of its four editors is a Soviet subject, another Hungarian, the other two are Americans, Professor de Solla Price

and Dr Garfield of the *Science Citation Index*. In the sample of four issues sent to me for review, 23 articles came from the United States, three from the Soviet Union, two from Canada and one each from Israel, the Netherlands and Hungary. Still, its international intention is to be welcomed.

Scientometrics gives every promise of being a journal of substance, constrained, perhaps even constricted, by its charter of quantification and its nearly overriding loyalty to a narrow range of sources — official statistics, the *Science Citation Index* and the study of the titles, authors and footnotes of published papers. It is easy to do very routine research on these sources and some of the articles already published show such signs. Professor de Solla Price has his task cut out; it is to be hoped that his past achievements will inspire in his authors similar standards.

FS is also international in the composition of its authorship and about half the articles are in French. It is published in England, its editors are French, its authors are from many countries. (The stylistic quality of the translations or of the editorial work leaves a certain amount to be desired.) Equally, the range of its subjects and techniques is very wide. Many of the articles seem to me to have a too pervasive polemical intention — overpowering in some, in which the entire repertory of the clichés of radical science is brought into play. Oddly enough, the more acrimonious expressions of this set of prejudices are appended to reports of meticulously executed investigations. In addition to these tendentious although interesting articles, there are a considerable number of serious and well-argued contributions on the philosophy of science. Nonetheless the editors would do well to

fundamenta scientiae

watch out for the arbitrary and ill-argued interpretations which some of their authors attach to otherwise interesting articles.

TS is less "scientific" than *Scientometrics*, less philosophical, less prejudiced and less polemical than FS with whom it shares the same publisher. Its charter is more specific than that of FS, more substantive than that of *Scientometrics* but it publishes too many papers in grooves already well travelled. This notwithstanding, articles by Harvey Brooks, Allan Bromley and one double-issue are of exceptional merit. For this, the editors had the good judgement and the good fortune to invite William Golden, a

sort of *éminence grise* of American governmental science policy, to edit a symposium on the making of science policy. He invited articles from all the former presidential science advisors, high officials of what is now the Office of Management and Budget, and other persons who have been intimately connected with the making of decisions about the direction and scale of the support of scientific research in the United States. The outcome, now also published as a book (*Science Advice to the President*), is very valuable.

If the editors can offer a bonus of this sort to their readers from time to time, they will compensate them for those articles

which repeat the commonplaces of "appropriate technology" and "science for development".

During the Second World War, there used to be signs in British railway stations asking "Is your journey really necessary?". It really is not an urgent journey to unknown places on which these three journals have launched themselves. But since they have done so, I wish them well. May common sense, discriminating judgement and an abhorrence of fashionable clichés accompany them on their way. □

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From the Cretaceous to the Planets

D.H. Tarling

Cretaceous Research. Editors-in-chief E.G. Kauffman and R.A. Reymont. 4/yr. (Academic.) £40 UK, \$105 elsewhere. *Open Earth*. Editor P.J. Smith. 4/yr. (P.J. Smith, 32 St James Close, Hanslope, Milton Keynes, UK.) £6.60, \$15.80 (personal); £9.90, \$23.70 (institutional). *Journal of Structural Geology*. Editor-in-chief P.L. Hancock. 4/yr. (Pergamon.) \$85. *The Moon and the Planets*. Editors A.G.W. Cameron *et al.* 8/yr in 2 vols. (Reidel.) Dfl.70, \$35 (personal); Dfl.233, \$116.50 (institutional).

THE four journals covered here are indeed a mixed bag, both in their scope and in their approach to their subject matter. All should be successful, albeit to varying degrees and for different reasons.

Cretaceous Research (CR) originated out of the need for publication of results from the IGCP Project 58 (Mid Cretaceous Events), and accepts contributions in both English and French on all aspects of the Cretaceous period. An abstract and key words appear with each article, with an additional English abstract for articles in French (why only French?). To maintain an international standing there are 16 editors, covering a wide geographical and academic range. Apart from standard articles, short communications and book reviews are included. There have also been useful features — "Bibliographia Cretacea", for example. CR carries six or seven articles in each issue, and the production of this journal is particularly good, with high-quality paper allowing excellent reproduction of photographs of, for example, microfossils.

Most journals do not concentrate on a specific period, and anything in this journal could equally well be published in either specialist subject or regional geological journals. A probable attraction for authors is the ability to publish local research in an international journal, so

they can reserve factors of wider import for publication in specialist journals. So far the editors have kept out very parochial papers, but it is unlikely that this can continue indefinitely. There are clear advantages to readers that material normally appearing in inaccessible journals is more readily available, but the apparent success of CR is likely to stimulate other journals — *Devonian Debates? Triassic Trends?* — and it is unlikely that most libraries would contemplate subscribing to periodicals for each stratigraphic period. Ultimately these are only likely to be purchased by individuals, with consequent reduction in quality and increase in price. At the moment it must be considered a good, but not essential, addition to an earth science library.

Quite different to all the other journals reviewed here is *Open Earth* (OE). It is more on the lines of *New Scientist* in containing quite academic articles but written lightly for an interested layman. Published and edited by Peter J. Smith, it is dominated by his influence and style. The size is similar to a news magazine, which indeed is what it is; OE is intended to provide information, with comments, on "happenings" within the earth sciences. Articles are rarely more than a page or two and most of its 40–50 pages are made up of short items of report, humour (of a sort!), book reviews, competitions and even geopoetry. Although available by air mail, it tends (but it is only a tendency) to be UK orientated.

As a news magazine, with typewritten print, it is an excellent publication. Highly readable, OE can be strongly recommended to any harassed earth scientist wanting to keep abreast of developments on a broad, albeit journalistic, level. It is highly suitable for the departmental coffee room as it is unlikely to be retained for reference.

Substantially thicker, generally over 100 pages per issue, but surprisingly of the same size as OE, the *Journal of Structural Geology* (JSG) fulfills a radically different role, being a specialist journal publishing original and review articles in structural geology and tectonics. In practice, it is closely complementary to *Tectonophysics*

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in concentrating on smaller scale deformations, although both journals cover all scales. As a British journal, JSG tends to have a European content rather than American, although this bias is doubtless not the intention of the widely dispersed editorial board of 22 and is likely to change. (Nonetheless it is noteworthy that many North American members of editorial boards are British in origin!) Articles are all in English, with an additional abstract allowed in a different language. The production is good, although the large pages do not seem to be used to full advantage and the paper quality is only just satisfactory for thin-section photographs. There have, so far, been too few review articles to make it essential reading for undergraduates, but the standard of papers is high and it is thus an important journal for inclusion in the geological section of a library.

Getting away from Earth, *The Moon and the Planets* (MP) is not new but rather re-born, as it is a continuation of an established journal, *The Moon*. MP is small in format, but quite well produced; the typeface seems slightly dense, but is readable and photographic reproductions come out quite well. It is interdisciplinary, concentrating on original research related to lunar and planetary bodies, although in practice largely chemically and physically orientated with surprisingly little on electromagnetic scanning. The articles are almost entirely in English, but other languages — French and Russian — have appeared, but only sometimes with an English abstract. There are generally eight to ten articles per issue, with a few letters, but no book reviews. An occasional bibliography appears.

Obviously the origin of this journal goes back to very early space studies and its change of content reflects the expansion of satellite exploration of the Solar System. MP is a highly focused publication, with few reviews or comparative papers, and although an essential journal for any institution with active space participation it is probably over-specialized for most libraries. □

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Time for Geomicrobiology

K.L. Temple

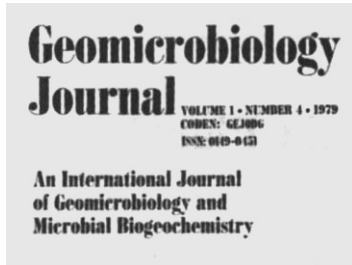
Geomicrobiology Journal. Editor H. Ehrlich. 4/yr. (Crane, Russak.) \$60.

The early years of *Geomicrobiology Journal* (GJ) have not been easy. Founded by Claude ZoBell, a notable name in the field, the journal was launched in 1978 but Vol. 2 was still incomplete in August 1981 — a bad sign.

Nonetheless such a forum was, and is, needed. Geomicrobiology papers are scattered throughout a number of established journals in diverse fields, and it was hoped that GJ would attract many of these. Unfortunately it has not yet done so; the quality of those papers that have appeared has been good, but too many of them have come from the editors.

With Vol. 3 the journal looks set for something of a re-birth. H. Ehrlich takes over as editor-in-chief and he will be supported by a large and wide-spread editorial board (14 of the 21 associate editors are from outside the USA). The list is truly distinguished; one expects high quality from such people. In addition to research reports, scientific reviews and book reviews, there will be letters to the editor, meeting announcements and discussion of previous papers. The format is good, the price reasonable and these new features should stimulate fresh interest in the journal.

With current concern over element cycling, curiosity about fossils and interest in old environments, GJ has a large potential audience. However, the casual reader in the USA will overlook the journal because the Library of Congress cataloguing department has seen fit to place it in a location remote from its associated fields of microbiology, sedimentology, geochemistry, palaeontology and ecology. Fortunately, I am told, this policy is to be changed.



Initiated by Beijerinck and Winoogradsky, geomicrobiology has just come of age. Theoretical advances, industrial and environmental applications and the increase in interdisciplinary research make it an appropriate time for this journal.

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What's New in Petroleum Exploration?

R.C. Selley

Journal of Petroleum Geology. Editor-in-chief E.N. Tiritsoo. 4/yr. (Scientific Press, PO Box 21, Beaconsfield, Bucks, UK.) £40.

THE advent of the new *Journal of Petroleum Geology* (JPG) is something of a nuisance. Previously the only essential journal for geologists engaged in petroleum exploration was the *Bulletin of the American Association of Petroleum Geologists*; one could generally be sure that any new petroleum exploration technique, or discovery of oil or gas in new regions, would shortly be reported within its pages. Unfortunately, the JPG publishes papers of such global scope and innovation that it, too, is now essential reading for any petroleum explorationist.

Each issue of the journal contains four or five papers, book reviews, discussions of previously published papers, reports of conferences and obituaries. The scope of papers is wide both geographically and in content. Few articles deal with North America, since this is covered by other journals, and fewer than one might expect

are concerned with the North Sea. Presumably the mighty conference volumes siphon off the majority of such papers. In so far as there is a regional bias, it is towards the Middle East, but the journal is essentially global in scope. The papers include reviews, technical research papers, and even humorous articles (Gold on earthquake outgassing, for example). The standard of production is high, and folding plates and colour maps occur in several issues. Since the dates of submission of papers are not given, it is not possible to ascertain the length of time that a paper is in press.

At an annual subscription of £36, and a rate of four 120-page issues per annum, the JPG will cost about 7½p a page for 1981. This can in no way compare with the value of the AAPG *Bulletin*; but it is not too different from some other geological journals, from whose subscription rates one might have thought the papers were not printed but embalmed.

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Climate Cubed

Ann Henderson-Sellers

Climatic Change. Editor S. H. Schneider. 4/yr. (Reidel.) Dfl.60, \$30 (personal); Dfl.153, \$77.50 (institutional).

"THIS is obviously the decade in which climate is coming into its own. You hardly heard the word professionally in the 1940's . . . It was clearly not the age of climate . . . Now it is!" Ken Hare's acknowledgement of a "new" discipline (*Bull. Am. Met. Soc.* 60, 1171–1174) was echoed by Steve Schneider, the editor of *Climatic Change* (CC) in the first issue — "Climatic change . . . no longer a subject for small talk".

In my opinion, CC achieves its goal of providing a medium for review and interchange. It is intended to be a new vehicle for a rapidly expanding discipline. Papers of this type were only rarely published previously, the journals probably most often cited being those of the American Meteorological Society and Geophysical Union. The three basic editorial criteria for this interdisciplinary publication are stated as disciplinary accuracy, clarity of cross-disciplinary communication and originality of content.



Practically all of the individual contributions (papers, reviews, editorials and correspondence) measure up to this stringent reviewers' policy set forth in the inaugural issue. Perhaps this success leads inevitably to the sensation of a potpourri — for example a recent issue contained papers on daily weather mapping from 1781; winter thunder and climate in China; American forests and climatic change; mechanisms of rapid deglaciation; and winds, rain and sunspots. In an editorial in the final issue of Vol.2, Schneider notes that as authors have become familiar with the editorial criteria, the rejection rate has fallen. It seems that speed of publication may also be reduced from the eight to ten months typical of Vols 1 and 2.

Steve Schneider once compared the climate to an 81-element cube having axes of space (local, regional, global), time (weather, climate and geological) and impact (physical, biological and social) — suddenly I recognize Rubik's cube! If this new journal is an attempt to "solve" the climate cube then the apparently random juxtapositioning of high-quality material is probably essential — it is certainly attractive. I wish the puzzlers luck.

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Pictures and Signals

P.W. Hawkes

Signal Processing. Editor M. Kunt. 6/yr. (North-Holland.) Dfl.220. *IEEE Transactions on Pattern Analysis and Machine Intelligence*. Editor T. Pavlidis. 6/yr. (IEEE Computer Society, Silver Springs, Maryland.) \$58 US, \$64 elsewhere.

SIGNAL processing is a very old subject, which has generated an extensive literature of books on signal theory and detection, stochastic processes, filtering and processing techniques, with time as the independent variable. Most of this is by, and addressed to electrical engineers, with contributions from pure mathematicians. In the past few years, the field has abruptly widened, with rapidly increasing interest in two (and higher) dimensional signals and in the application of processing methods to subjects that are not primarily the province of electrical engineers.

It is to a mixed audience that these two new journals, *Signal Processing* (SP) and the *IEEE Transactions on Pattern Analysis and Machine Intelligence* (PAMI), are addressed. Both contain papers, short communications or letters, and book reviews (PAMI) or "alerts" (SP). Both are attracting much the same kind of paper, though SP includes material that would probably have been sent to another of the *IEEE Transactions* series — a recent discussion of the "design of high order minimum phase FIR digital or CCD filters" is an example of this. SP also contains a sprinkling of papers on one-dimensional signals, which rarely occur in PAMI.

PAMI has published many interesting papers in the field of image processing and pattern analysis since its first appearance in 1979 — on Hough and Radon transforms, on the use of moments, on the use of variable knot bicubic splines for image approximation, on mosaic models for textures, on the use of fuzzy set decomposition to assess clustering performance: these give a fair idea of the flavour of the contents. By contrast, SP is often more down-to-earth: recent papers have covered speech analysis by pole-zero decomposition of short-time spectra, a set of optimal discrete linear smoothers, a graphical system for geographical data processing and medical scintigraphic images.

IEEE TRANSACTIONS ON PATTERN ANALYSIS AND MACHINE INTELLIGENCE

Are these two new journals needed? Probably not, for the longer-established *Computer Graphics and Image Processing* has quite as distinguished an editor and editorial board as either of them, and would have been a suitable home for many of the papers in SP and most of those in

PAMI. Moreover, there are already *IEEE Transactions* on closely related topics (series C, COM and ASSP in particular), and also such specialized titles as *Pattern Recognition* and *Photogrammetria*. Nevertheless, these newcomers cater for a rapidly expanding field and anyone working in it will have to have access to PAMI and probably to SP as well. □

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Solar Energy Foci

J.I.B. Wilson

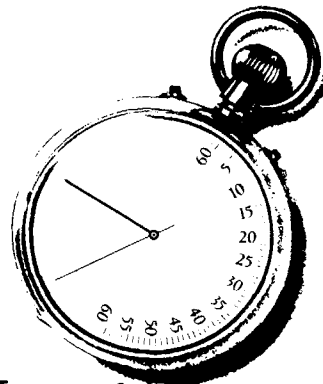
Solar Energy Materials. General editor B.O. Seraphin. 6/yr in 2 vols. (North-Holland.) DG 360. *Solar Cells*. Editors-in-chief T.J. Coutts *et al.* 8/yr in 2 vols. (Elsevier Sequoia.) SwFr. 380.

THE enthusiasm for "self-sufficiency" and "alternative energy" has generated an abundance of pamphlets and books, as well as many (often short-lived) magazines, but the official journal of the International Solar Energy Society, *Solar Energy*, has remained until recently the only place for the dissemination of most scientific and technical developments. Consequently, much useful information has only been available in obscure house journals, or has been published in such places as the many electronics journals. It has been especially difficult to keep up with progress in photovoltaic and photochemical technology without attending the over-large international conferences, and papers which combined economics with natural sciences have not been easy to place.

Now two specialist journals have arrived to service the field, which is becoming divided into photochemical and photo-thermal conversion — *Solar Energy Materials* (SEM) — and photovoltaic conversion — *Solar Cells* (SC) — whilst *Solar Energy* itself has evolved into a mechanical engineering and systems forum. Although both of the new publications have a similar format, style and issue size, there are differences in cost and in the type of article which appears. The delay between acceptance and appearance is around six months in both, although some papers in SEM are undated.

In addition to technology, SC contains many theoretical analyses of solar cells as energy converters, as well as some interesting economic evaluations which are, after all, the final test of success of any new energy source. In contrast, SEM has been concerned mainly with the "recipe" type of paper, such as the preparation of selective coatings for thermal collectors. The combination of photothermal conversion, which is already economically viable (unlike solar cells), and photochemical

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conversion, which is still a long way from the efficiency and operating lifetime required, may be an uneasy one, but neither of them uses the sophisticated materials preparation required by most solid-state devices. However, because of the similarity of operation of the non-thermal "quantum converters", I believe it is the intention of the editors of SC to relax the usual narrow sense of solar cell, meaning photovoltaic cell, to include, for instance, photoelectrochemical cells.

A useful feature of both journals is their publication of review papers, and the coverage of otherwise unreported workshops. Fifty per cent of the papers in each are from the USA, and another 30 per cent from Australia (reflecting the effort there into thermal collectors for high temperatures), otherwise the contributors are spread worldwide. Many papers are appearing in these new periodicals at the expense of established publications, but undoubtedly more new papers reach their intended audience.

The fortuitous separation of solar energy coverage into three or so divisions should assist librarians and individuals in their choice of subscription, but all three of the journals referred to are essential for any serious solar energy programme. □

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Remote Sensing and Spatial Information

Robert N. Colwell

International Journal of Remote Sensing. Executive editor Y.C. Robertson. 4/yr. (Taylor & Francis, London.) £39, \$105, DM187. *Geo-Processing: Geo-Data, Geo-Systems and Digital Mapping.* Editor-in-chief T.K. Peucker. 4/yr. (Elsevier Scientific.) DG103.

REMOTE-sensing-orientated scientists will welcome these two new journals, both of which provide prompt publication of highly relevant papers, reviews of books and periodicals, and thought-provoking editorial comments.

The *International Journal of Remote Sensing* (IJRS) deals with the gathering, interpretation and use of remotely-sensed data. Emphasis is placed on, first, technological developments in sensors, image processing, image analysis and the display of remote-sensing-derived information; and, second, discussion of the principles and procedures that are entailed in various phases of the work done by remote-sensing scientists. The papers published thus far have shown a consistent balance between pure and applied research activities. The highly diversified remote-sensing-related fields continue to be covered, topics ranging from agriculture to meteorology, from forestry to urban planning, and from hazard monitoring to cost-benefit analysis.

GEO-PROCESSING

Geo-Processing (GP) seeks to serve the interests of all groups that deal, in one way or another, with spatial information systems. Collectively, the articles published to date have dealt primarily with the construction of regional, national and global information systems, and with the acquisition and analysis of both remote-sensing-derived data and ancillary data that contribute to such systems. Through this focus, GP appears to provide a much-needed home for certain papers which heretofore have been relatively dispersed through the literature. The principal systems dealt with are those applicable to cadastral geocoding, thematic and topographic mapping, the inventory and monitoring of various kinds of natural resources, and the construction of digital terrain models. Partly because articles are submitted by the authors as camera-ready copy, are printed by direct offset reproduction, and contain few, if any, half-tone illustrations, quite appropriately the subscription cost for this journal is much less than that of the comparably-sized, but more attractively produced, IJRS.

Since both of these journals provide, in English, a large amount of current information dealing with highly important facets of remote sensing and its related

disciplines, they offer good value to a wide spectrum of subscribers and appear to be complementary to, rather than competitive with, other journals which deal with similar subject matter such as *Photogrammetria*, *Remote Sensing of Environment* and *Photogrammetric Engineering and Remote Sensing*. □

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Superficial Science

John Gregory

Journal of Dispersion Science and Technology. Editors-in-chief S.E. Friberg and P. Becher. 4/yr. (Dekker.) \$60. *Colloids and Surfaces.* Editor-in-chief P. Somasundaran. 8/yr in 2 vols. (Elsevier Scientific.) DG390.

PRIOR to the appearance of these two new journals, their fields of interest were dominated by the *Journal of Colloid and Interface Science* (JCIS), founded by V.K. La Mer in 1946, and, so far, showing no sign of being in the least perturbed by the newcomers. Other major outlets for work in the area are *Colloid and Polymer Science* (published in Germany, but with most papers in English) and *Colloid Journal*, a Russian journal which appears in a cover-to-cover English translation.

Owing to a rapid growth of activity in colloid and interface science, pressure on space in JCIS had become intense, with long publication delays despite an increasing amount of material published. There is little doubt that the *Journal of Dispersion Science and Technology* (JDST) and *Colloids and Surfaces* (CS) were launched with the hope of creaming off some of the material which would otherwise have gone to JCIS. In such circumstances, it was vital for the new contenders to establish distinct identities and to attract high-quality papers — not just those unable to get past the JCIS referees. On present form, both new journals appear to be succeeding in their different ways; indeed, CS has already increased its rate of publication from quarterly to eight issues per year.

The crucial problem of filling the first few issues with high-calibre papers was solved in JDST by soliciting papers from members of the Advisory Board and Editorial Committee. Similar tactics were employed by the CS editor, who had the added advantage of several good papers presented at a National Colloid Symposium in 1979.

The pattern now emerging for JDST — a

rather unattractive, but relatively cheap, camera-ready production — is a heavy concentration on papers dealing with surfactants, emulsions and micro-emulsions. Because of the increasing industrial importance of these topics, especially in the oil recovery area, this may be a policy worth pursuing, although in that case a more appropriate title for the journal might be desirable. The quality of papers so far has been maintained at a reasonably high level and several useful short reviews have appeared. Dates of receipt and acceptance of papers are not given so I cannot judge the speed of publication, which is one of the aims of JDST. For those working in the emulsion/surfactant area, a personal subscription to this journal might well be worthwhile.

In the case of CS, well produced and largely typeset conventionally, a much wider range of topics is covered — very like that of JCIS in fact, so that comparisons between the new journal and the established leader in the field are inescapable. So far, it appears that CS is attracting papers which represent a rather

more even balance between fundamental and applied aspects of the subject than JCIS, which is mainly concerned with fundamentals. The papers are mostly good, and some excellent contributions have been published. The time from acceptance to publication seems to be mainly in the range seven to ten months. Brief notes (from camera-ready copy) are published more rapidly, together with book reviews and conference announcements.

Whether or not they felt a need for one, colloid scientists will just have to accept the fact that, in *Colloids and Surfaces*, they now have another broadly-based journal of high quality, which is rapidly becoming indispensable. Persuading a library committee to approve yet another subscription is, of course, a different matter. □

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New Solids: Research for Application

Arthur T. Howe

Solid State Ionics. Principal editor M.S. Whittingham. 9/yr in 3 vols. (North-Holland.) DG480. *Synthetic Metals*. Editor F.L. Vogel. 4/yr. (Elsevier Sequoia.) SwFr.360.

IT WAS known before the turn of the century that the silver ions in solid silver iodide are not static, but behave like a liquid. However this compound, and its derivatives, remained much of a curiosity until the late 1960s when more examples were discovered, heralding the Renaissance of the field which has up till now produced fast ion conductors (sometimes called superionic conductors) for a number of elements in solids — hydrogen, lithium and sodium for example. The potential application of these materials in new batteries has injected an urgency into their study, which prompted the launching of *Solid State Ionics* (SSI) in 1980.

Particularly in the light of long publication times in the *Journal of Solid State Chemistry*, the more general journal appropriate to the field, SSI has fulfilled its aim of rapid publication, with times for papers typically less than four months in the latest issue, with the special section for notes offering even shorter times. Speed should only be an adjunct of quality, however, and SSI has already attracted an impressive collection of significant papers in the field. In addition the publishers have managed to keep a pleasing, properly printed format. Certainly, I have found a

subscription essential, and I am sure that the journal will emerge as the major organ of this important field.

In contrast to the commendable speed of publication in SSI, contributors to *Synthetic Metals* (SM) currently must wait a considerable time — 10 to 18 months — to see their papers in print. Nonetheless this journal, too, looks set for a healthy future. SM is well produced and, as well as publishing papers and reviews, frequently features collections of papers presented at conferences.

The journal was designed to highlight the growth of interest in the areas of intercalation compounds of graphite, transition metal compounds and quasi one-dimensional conductors. The term "synthetic metals" was coined to describe those materials which possessed metallic conductivity but were not the normal elemental metals. Conducting films of polyacetylene, (CH)_x, first synthesized in 1974, sparked off the search for plastic conductors to replace copper and aluminium. SM has provided a much-needed forum for the explosion of activity in this truly multidisciplinary area — for example, the spread of the journal is indicated by reports in a recent issue on Raman, ion implantation, microscopic, thermopower and conductivity experiments. □

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Angles on Surfaces

D.P. Woodruff

Surface Science Letters. Editor H.C. Gatos. 24/yr. (North-Holland.) \$30 (personal only). *Surface and Interface Analysis*. Editor-in-chief D. Briggs. 6/yr. (Heyden.) £82, \$180, DM 382.

WHILE much of the literature on fundamental studies of surfaces is dispersed through a variety of journals, *Surface Science* has established itself as the mainstream journal for such work and has expanded its scale to a volume (in two parts) of about 700 pages each month. While this journal has long had a "letters" section, their publication has not been especially speedy and the cost of the journal lies far outside the budget of individual researchers. *Surface Science Letters* is not a new journal as such, but an attempt to revitalize the letters section of its parent by making this section available to private subscribers at relatively modest cost. As such it is a very worthwhile venture; publication delays are still a little long (6 months or so) but the availability of this new format is encouraging the convergence of more short papers and speculative ideas into a single periodical which is guaranteed a large audience.

By contrast, *Surface and Interface Analysis* (SIA) is an entirely new journal of modest proportions (each issue is about 40 large-format pages) which attempts to fill a new position in the literature. Many of the new techniques which have emerged in the past 15 years of fundamental surface science have proved to be of considerable value in the elucidation and characterization of materials' problems related to surface and thin films (including interface) phenomena. The publication of work on these applications has been dispersed in a range of journals. To some extent, this situation has advantages; the specific application may be of far more interest to those concerned with the relevant material (for example, glasses, catalysts, semiconductor devices) than to those applying the same technique to different systems. On the other hand, many problems are common to the technique — quantification, instrumentation and calibration, for example — and a common forum for these is clearly valuable. SIA is attracting such papers along with a number of contributions on specific applications.

Undoubtedly the journal does fill a need, although the size of this valuable core of literature is small and could not support a larger journal. In principle, another relatively recent journal, *Applications of Surface Science*, has a similar objective; in practice, however, it has failed to distinguish itself sufficiently in content from its sister journal, *Surface Science*.

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Polymers, Pure and Applied

Paul Calvert

Polymer Bulletin. Editors H.-J. Cantow *et al.* 24/yr in 2 vols. (Springer-Verlag.) DM256. *Polymer Testing*. Editor R. Brown. 4/yr. (Applied Science, Barking, UK.) £43. *Polymer Degradation and Stability*. Editor N. Grassie. 6/yr. (Applied Science, Barking, UK.) £69.

POLYMER science has been well served for "archival" journals over the years. The *Journal of Polymer Science* (JPS) started in 1946 and has now expanded to five sections. In recent years its dominance has waned as *Polymer* and *Macromolecules* have appeared and thrived. Currently, the (synthetic) polymer science section of a library would need to take about five polymer journals to have a reasonable coverage of the field (counting JPS as one); another five or so could be regarded as useful but not vital.

Polymer Bulletin (PB) and *Makromolekulare Chemie — Rapid Communications* (not reviewed here) have both appeared during the past three years and are the first fast-publishing journals in the field. PB takes short papers of four to six single-spaced, camera-ready pages. At the moment publication is taking one or two months from receipt of the paper to cover date of the issue, compared with about six months in the venerable Letters Edition of JPS and one to two years in many of the "full" journals (much of this long hiatus seems to be in refereeing).

Polymer Bulletin

The reader does not have a great deal to gain from very rapid publication. Research results in polymers remain "new" for a year or two and journals containing large numbers of short papers make very boring reading. However, for authors, rapid publication has advantages. Replying to referees' comments is easier within the first couple of months after a paper is finished and it is much better if a paper is published while the author can still remember what it contains. In principle one can now quickly publish brief progress reports and put more effort into the occasional archival gem. Since many of us seem to spend more time as authors than as readers, both of these rapid-publication journals should be successful. However, they could probably be improved from the reader's viewpoint by dividing the contents into sections by subject.

As our "purer" colleagues in physics and chemistry often point out, polymer science is really an applied science and much work in the field is done in industrial research and development laboratories and

in a variety of research associations, standards laboratories and military laboratories. The scientific journals serve well the pure scientists, while the "trade press" serves the plastics engineer, but in the middle there is a gap filled only by one or two applied polymer science periodicals.

POLYMER TESTING

Polymer Testing (PT) falls into this middle region; most members of the editorial board are from company or government laboratories. Full length papers are published on methods of testing and standardizing polymers; initially paints and adhesives were excluded but are now allowed by popular request. This journal also differs from the others in having a distinct personality, with an editorial and occasional readers' letters.

Automation, Atoms and Analysis

T.S. West

The Journal of Automatic Chemistry. Editor P.B. Stockwell. 4/yr. (United Trade Press, London.) £30 UK, SwFr.161, \$80.50 elsewhere. *Progress in Analytical Atomic Spectroscopy*. 4/yr. Editor-in-chief C.L. Chakrabarti. (Pergamon.) \$85.

THE automation of laboratory techniques and the emergence of applied atomic spectroscopy are two of the most significant developments in analysis during recent years. Given the sharp eyes of publishers for fresh ventures, it is not surprising that two new review journals have appeared which deal with precisely these topics.

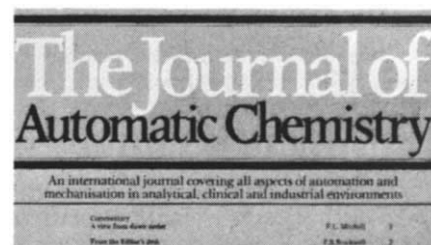
The international *Journal of Automatic Chemistry* (JAC) covers all aspects of automation and mechanization in analytical, clinical and industrial laboratories. These are areas in which automation has become absolutely essential because of the necessity to handle enormous numbers of samples and to cut down costs (the manual operator is perhaps the most expensive "instrument" in all laboratories). Each issue contains about eight or nine articles, many of them devoted to microprocessors and a smaller number to improvements to commercial automated systems or the construction of miscellaneous ancillary devices to improve their performance. In addition there are useful and informative articles on meetings, new products and literature, and

The readership for PT is secure because many laboratories are concerned with the testing and quality control of plastics. Authorship is likely to be more of a problem in that industrial scientists do not publish much — papers in the current issues still rely heavily on institutions represented on the editorial board and it may take some time for the journal to expand beyond this.

A "stablemate" of PT, *Polymer Degradation and Stability* (PDS) has an editorial board which is almost completely drawn from academic and government research centres. The journal publishes full papers with a publication lag of six months to one year, which is about as fast as one can expect for a quarterly journal. There are not many institutions which do basic research on polymer degradation, so PDS is really aimed at quite a small community. For those in the field the narrowness of the topic makes it an interesting (and easy) journal to browse through. If it can survive with such a small potential readership it will do a useful job. □

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a running calendar of current and forthcoming events. There are also occasional book reviews.



The standard of the papers is usually good, even excellent, and the "virtual" A4 format allows good use to be made of illustrations. The generally high quality of the articles read in the sample issues suggests that this journal has so far done an excellent job and should be scanned on a regular basis by all who are concerned with automation. It is not possible, since the dates are not given, to establish publication time, but the journal gives the impression of being very much "on the ball".

The blunt intention of the editor of *Progress in Analytical Atomic Spectroscopy* (PAAS) is "to fill a serious gap in the literature of a rapidly expanding field". Each of the three other issues sampled since the first, which appeared virtually to be the complete publication of a PhD thesis (with no other papers), contains two to four review papers written by specialists in particular areas by invitation of the

Editorial Board. Publication from camera-ready manuscripts appears to offer rapid publication, but no dates of receipt are given on the articles.

This journal in no way competes with the Royal Society of Chemistry's *Annual Reports on Analytical Atomic Spectroscopy* which are devoted to fairly comprehensive reviews of all activity in the field during the previous year. Instead, PAAS gives in-depth treatment to small areas or specific topics, or critical surveys of the application of these techniques to specific industries, for example nuclear energy materials, cement, refractories, paints, soils and plants. The juxtaposition of reviews on various industrial analyses and others of a fundamental academic nature is at first a little disconcerting, but it does perhaps substantiate the observation that the field is one which is expanding rapidly.

I cannot honestly say that this journal fulfills the same unique role as JAC, since quite a few of its articles might equally well be found elsewhere and there is consequently no characteristic "flavour" to the journal. But what PAAS does outstandingly well is to offer the opportunity for publication of uniquely long and detailed fundamental papers, and if this aspect can be further developed the journal will undoubtedly attain a character of its own and serve a vital role.

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Transitions in Solid-state Physics

Roger Pynn

Phase Transitions. Editors-in-chief A.M. Glazer and R. Smoluchowski. 4/yr. (Gordon & Breach.) 1981: \$107.

THE avowed aim of *Phase Transitions* (PT) is to provide "a focus for papers on most aspects of phase transitions in condensed matter". Yet it is not obvious that such a focus is necessary or even desirable. During the past decade of expanding interest in phase transitions, the established condensed-matter journals have provided an adequate forum for papers on phase transitions and have maintained a measure of contact between students of this sub-discipline and the remainder of the solid-state physics community.

Although papers published to date in this new journal have been of a quality which is above average, none can be described as seminal. The editors have tried to improve the refereeing system by keeping authors' names and affiliations

Inorganic Chemistry for the Broad-minded

J.A. McCleverty

Reviews in Inorganic Chemistry. Editors H.D.B. Jenkins and D.F.C. Morris. 4/yr. (Freund, Tel Aviv, Israel/Wiley.) \$85.

IN THEIR preface, the editors of *Reviews in Inorganic Chemistry* (RIC) make a point of inviting short, authoritative articles which will be seen as personal reviews of the author's own research set in the context of other work in the field. Larger and more broadly based reviews are also solicited, and in recent issues book reviews are included.

Thus the policy of the journal is to encourage new insights and to promote interest and research activity across the span of inorganic chemistry. The editors apparently hope to achieve this by placing no restrictions on what constitutes inorganic chemistry in order to promote the interplay of ideas between subdivisions of the subject. Certainly, a wide variety of topics have been reviewed to date, ranging from basic coordination chemistry, through thermodynamics, spectroscopy, electrochemistry and chromatography, to solid-state chemistry.

There are short personal accounts and longer more general contributions of variable quality, but some useful articles by well-known chemists have also appeared. The delay between receipt of manuscripts and their appearance has apparently decreased since the first issue, and seems reasonable. Thus the editors appear to be achieving some of their aims, one of which

is rapid publication.

It seems unlikely that individuals would buy RIC, but departments having a broad range of inorganic interests might. The price is not excessive and the quality of production quite satisfactory. However, in my opinion the journal does not fill an obvious gap; rather, I would guess that it tends to siphon off articles from more specialized review periodicals. I would place it in the middle rank of quality, certainly below its Wiley stablemate, *Progress in Inorganic Chemistry*.



RIC, then, is not without worth. But a warning note is sounded by the fact that although it was launched in January 1979 with a quarterly publication schedule, only eight issues have appeared to date.

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Disposal Technology

R.I. Newman

Nuclear and Chemical Waste Management. Editors-in-chief L. Cranberg and A.A. Moghissi. 4/yr. (Pergamon.) \$80. *Radioactive Waste Management.* Editor-in-chief D.R. Anderson. 4/yr. (Harwood.) Dfl.150, \$55. *European Applied Research Reports: Nuclear Science and Technology Section.* Irregular. (Harwood.) Dfl.855, \$310 per volume of 1,500 to 1,600 pages.

THE appearance of three new journals dealing with the management of radioactive waste, at least in part, is something of an occasion. Among the criteria for the success of a new journal is that it should fill a gap, and there is certainly a need for a forum for the objective discussion of waste management problems. Thus I took up with optimism the task of reading the 40 or so papers and the accompanying editorial statements in the first four issues of each of these journals.

The statement of editorial policy in *Radioactive Waste Management* (RWM) is entirely to my liking. I applaud the editors' intentions to be "skeptical of submissions that continue to press for a 'best' instead of acceptable solution(s) to technical, political or social problems". And it is good that *Nuclear and Chemical Waste Management* (NCWM) quotes from a statement by the Coordinating Committee on Energy to the effect that "Nuclear waste

from the referees, an interesting idea which could be adopted profitably elsewhere; some established journals often appear to give undue prominence to a limited number of laboratories. Although fair, the refereeing is often not particularly rapid — overall, the delay between the submission of a paper and its appearance varies between six months and more than a year. This situation is exacerbated by the relatively infrequent publication schedule (about four issues per year).

The quality of production of the journal is excellent and the lack of page charges and of length limitation seems to have encouraged papers which are self-contained and well expounded. However, to flourish beyond its infancy PT must attract, in addition, a few landmark contributions and establish a reputation in this specific area of study.

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and radioactive effluents can be safely managed with existing technologies or a straightforward extension thereof" and that "the waste disposal issue is an important part of the effort [to assist the expansion of nuclear power] and one that does not require extraordinary measures".

Part of the reason why these truths have not been more widely appreciated is perhaps the United States government's indecisiveness in setting rational limits and criteria for the disposal of radioactive waste. (There is no shortage of designs.) The result has been a proliferation of research and development projects, usually leading to the recommendation that even more research and development is needed. Quixotically, people have set out not merely to find the "best" method or materials, but to pile several "bests" upon each other.

Unfortunately, many of the papers in these three journals are in that unrealistic tradition. My estimate is that only some 15 out of the 40 papers support the adequacy of current technology. Half a dozen of these papers, however, are outstanding — they deal with matters such as the adequacy of glass for nuclear waste disposal and the economics of waste disposal as part of the entire fuel cycle.

Although each of these journals deals with the problems of the disposal of radioactive waste, their emphasis varies. Thus, while RWM is devoted entirely to such questions (with concentration on

proceedings are apparently published from time to time.

All three journals deserve a place in libraries concerned with the topic of waste management even though, as always, it will be necessary for readers to sift for themselves the wheat from the chaff. □

R.I. Newman is a consultant specializing in nuclear fuel reprocessing and radioactive waste disposal, and is Chairman of the Nuclear Engineering Division of the American Institute of Chemical Engineers.

Rapid Damage

C.A. English

Radiation Effects Letters. Editor-in-chief L.T. Chadderton. 54/yr in 9 vols. (Gordon & Breach.) 1981: \$157.

Radiation Effects Letters (REL) was launched with the aim of facilitating the rapid publication of short contributions on the effects generated by the interaction of radiation with matter. It invites both experimental and theoretical contributions over the same wide subject range as its well-established companion journal, *Radiation Effects*. The latter remains the vehicle for full-length papers, correspondence and book reviews.

Topics covered in both journals range from the interaction of radiation with crystalline materials to radiation effects in biology and biochemistry. To aid quick publication, REL requires that letters of up to six pages are submitted in camera-ready form suitable for direct reproduction; although the quality of production is good, all letters appear to be published without revision and as a consequence some lack clarity. The standard of contents is variable, and it is difficult to be convinced that all letters represent truly significant contributions requiring rapid publication. The majority, however, provide information on topics of current interest, and thus are of relevance to workers in this area.

Typically, an issue contains articles received over a six to eight week period. Thus time to publication is faster than that previously achieved by short communications in its parent journal, and compares well with other journals offering rapid publication of letters. However, about a third of the issues of REL have taken six to nine months to reach Harwell. It is to be hoped that this problem is not widespread as it negates the main purpose of the journal.

The appearance of REL must be welcomed. The journal fills a potentially valuable role in allowing the speedy dissemination of information on topics of current interest in its chosen area. □

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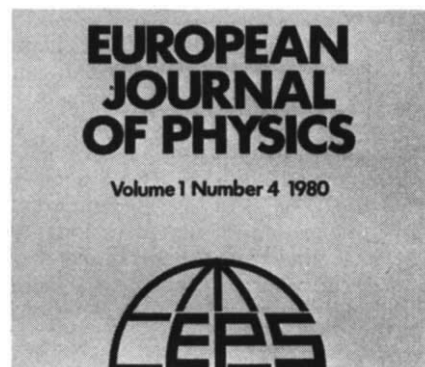
Teaching Physics

Daphne F. Jackson

European Journal of Physics. Editor G.W. Series. 4/yr. (Institute of Physics, Bristol, UK.) \$80 US, £35 elsewhere.

THIS journal aims to provide a European equivalent of the *American Journal of Physics*, which plays an important part in the literature of physics education, chiefly as a vehicle for novel insights into points of physics which are often prompted by the experience of physics teachers at all levels.

The *European Journal of Physics* (EJP) is solely concerned with explicitly educational questions. The editor identifies four broad areas for contributors, and the journal publishes articles



(mostly in English) on particular topics in physics or teaching methods at university level; on the fundamentals of physics or new insights into known areas of physics; on topics which cross the boundaries between physics and other disciplines; and on the cultural implications of physics.

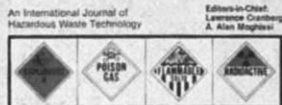
Each issue contains 60–70 pages of papers (not normally longer than 5,000 words), book reviews and reports on conferences and other events; letters and comments, including comments on previously published papers, are also accepted but are subject to the usual refereeing procedure. Publication appears to be rapid.

Much thought and effort has evidently been put into the launching of the journal, and the standard of production is generally good; the text and mathematical content have a clean, simple appearance, and the illustrations are well reproduced.

Almost all the contributions in the four issues of the inaugural volume fall into the first two categories mentioned above. But whereas the articles on teaching methods are not very exciting and could have appeared elsewhere, those on fundamental topics or controversies in physics are of greater depth. As a forum for papers of this kind, together with developing sections on cross-disciplinary and cultural topics, the EJP could prove timely and valuable. □

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Nuclear and Chemical Waste Management



waste from the production of nuclear energy), NCWM also covers the disposal of hazardous chemicals. The brief of *European Applied Research Reports* (EARR) is broader still: it publishes "reports on all aspects of nuclear science and technology", a significant proportion of which deal with technical approaches to nuclear waste management. Conversely, EARR accepts contributions from a geographically more limited area than either of the other two journals, since it only accepts papers which have been sponsored by, or published in collaboration with, the Commission of European Communities.

Within their self-restricted subject areas, NCWM and RWM carry a variety of material. They deal with economics, safety and public policy as well as with technical matters. Both publications include editorials and letters to the editor, of a uniformly high quality in the first few issues. EARR is evidently not intended as a "talking shop" since it carries only technical reports, although symposium

Fast Reactions

Frank Close

Zeitschrift für Physik, C: Particles and Fields. Editors G. Kramer and H. Satz. 20/yr in 5 vols. (Springer-Verlag.) DM1,140. *Surveys in High Energy Physics: An International Journal*. Editor-in-chief J.M. Charap. 4/yr. (Harwood.) \$52, £25.

SPRINGER-Verlag have long had a high reputation in producing scientific literature and for some years have published Parts A and B of *Zeitschrift für Physik (ZP)*, *Atoms and Nuclei and Condensed Matter*. Part C, a recent addition, is dedicated to "particles and fields", which primarily means high energy physics.

In the USA most papers in this subject area are submitted to *Physical Review*. Although this carries the highest reputation, in times of financial stringency European authors are often unable to publish there as a result of page charges. Thus *Nuclear Physics*, Part B, has had a monopoly on the European scene in recent years, publication delays tending to deter from publishing in *Nuovo Cimento*.

Although *Nuclear Physics* remains the primary European journal in high energy physics, the new part of ZP has rapidly established a high reputation. This has been helped by the fact that physicists at the electron-positron collider (PETRA) at Hamburg have, until recently, worked alone at this frontier of science.



The German connection has made ZP a popular choice for researchers at Hamburg; in consequence high energy physicists are reading it, and in turn publishing there. Its reputation is rapidly approaching that of *Nuclear Physics* for publishing front-line research with a tendency to phenomenological rather than "theoretical theory". The original hope for rapid publication has fallen a bit recently, but still compares favourably with similar journals. The quality of production and of contributed papers are good, and its well-being seems assured.

In contrast, the future of *Surveys in High Energy Physics* (SHEP), devoted to theoretical high energy physics including surveys of experimental results, does not appear to be so bright. The articles are often lengthy and of a review nature, rather than the first reporting of the results of research problems. The journal aims to deal with ideas that are "timely rather than merely topical so that issues are not rapidly outdated or disproved". To a fair extent the editors have succeeded here: the first issue contained a review of jet behaviour in

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electron-positron annihilation, and a survey of theory on QCD jets was in the following number. Camera-ready copy should speed up publication and help keep down costs (though the early volumes are costing around \$50 each: it is not clear to me how long librarians will regard this to be a reasonable buy). The journal is also available in microform.

A new journal needs a period of time to

become established, after which it becomes indispensable. I have lived happily without SHEP for several years and did not find many colleagues who read it. I fear that when library budgets tighten it may be found to be a luxury.

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ARTICLES

Dilute (*d*) coat colour mutation of DBA/2J mice is associated with the site of integration of an ecotropic MuLV genome

Nancy A. Jenkins, Neal G. Copeland, Benjamin A. Taylor & Barbara K. Lee

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*The single endogenous DBA/2J ecotropic provirus segregated concordantly with the dilute (*d*) coat colour mutation on chromosome 9 in 53/53 DBA/2J-derived recombinant inbred mouse strains and all seven inbred and mutant strains tested that carry the *d* allele. Analysis of DNA from a spontaneous DBA/2J *d* revertant (*d*^{+2J}) showed that these mice lack ecotropic-specific MuLV DNA sequences and suggested that the dilute mutation resulted from integration of an ecotropic provirus into the mouse genome.*

ECOTROPIC murine leukaemia virus (MuLV) DNA sequences are inherited as part of the normal genetic complement of many inbred strains of *Mus musculus*. Several high leukaemic strains such as AKR, C58, and PL spontaneously produce high titres of ecotropic MuLV early in life and carry multiple (3–5) copies (per haploid genome) of ecotropic viral DNA sequences (refs 1–3 and N.A.J. *et al.*, in preparation). In contrast, mice of many low leukaemic strains including DBA, BALB/c, C57BL/6 and C3H/He express ecotropic MuLV only on aging or chemical induction³ and contain a single copy (per haploid genome) of ecotropic viral DNA sequences^{1,3}.

Although the structure of most ecotropic MuLV genomes in high and low virus-producing strains is similar to the prototype Gross-AKR MuLV^{1,4}, the endogenous ecotropic MuLVs are integrated at multiple sites in mouse chromosomes. In the AKR/N strain, two independently segregating loci for ecotropic virus inducibility have been mapped^{5–7} to chromosomes 7 and 16. The single ecotropic virus inducibility locus of BALB/c mice has been assigned^{8,9} to chromosome 5 and may be allelic with the C3H/He ecotropic provirus⁹. The ecotropic virus inducibility locus of C57BL/6J is unlinked to those in C3H/HeJ and BALB/cJ¹⁰ and has been assigned¹¹ to chromosome 8.

We have determined the chromosomal location of the DBA/2J ecotropic provirus, *Dbv*, by Southern blotting and hybridization with an ecotropic-specific DNA probe of the DNAs from two sets of recombinant inbred (RI) mouse strains derived from crosses of DBA/2J with either C57BL/6J (the BXD series) or AKR/J (the AKXD series). RI strains are derived by systematic inbreeding, beginning with randomly chosen pairs of mice from the F₂ generation of the cross of two pre-existing (progenitor) strains¹². RI strains are particularly useful for gene mapping because, during inbreeding, linked genes tend to remain linked and unlinked genes are randomized in the RI strains. The results of our analysis (1) confirm that the endogenous ecotropic provirus of DBA/2J is unlinked to those in C57BL/6J and AKR/J, (2) indicate that the endogenous ecotropic proviruses of these strains are structurally indistinguishable from the exogenous AKR ecotropic provirus, and (3) demonstrate that the DBA/2J ecotropic provirus is closely linked to the dilute (*d*) allele on chromosome 9. This linkage was confirmed by analysis of other inbred strains and a spontaneous *d* allele revertant of DBA/2J, designated *d*^{+2J}.

Characterization of ecotropic MuLV DNA sequences in BXD RI strains

We have used Southern blotting and hybridization with an ecotropic-specific DNA probe to characterize the endogenous

ecotropic DNA sequences in 26 BXD recombinant inbred and 2 progenitor strains. The hybridization probe, representing a 400-base pair (bp) *Sma*I fragment from the *env* gene of AKR MuLV DNA subcloned into the *Eco*RI site of plasmid pBR322 (Fig. 1), hybridizes only to ecotropic DNA sequences (both N and B ecotropic MuLVs) and not xenotropic, amphotropic or mink cell focus-forming (MCF) MuLVs¹ (this probe was provided by Dr D. R. Lowy).

Initially, we examined the organization of ecotropic MuLV genomes in the two progenitors of the BXD RI strains. DNA was prepared from spleens of C57BL/6J and DBA/2J mice, digested to completion with several different restriction enzymes (Fig. 1), electrophoresed through agarose gels, blotted onto nitrocellulose paper and hybridized with a ³²P-labelled nick-translated probe. Digestion of C57BL/6J and DBA/2J DNA with enzymes that should generate a single internal viral DNA fragment which is detectable with this probe (*Pst*I, *Kpn*I or *Bam*HI) in all cases produced a single fragment that co-migrated with the analogous fragment in the prototype AKR viral DNA (data not shown). In addition, digestion of these DNAs with *Eco*RI, *Pvu*II, or *Xba*I produced single detectable viral DNA-containing fragments which differed in size in these two strains and were larger than the corresponding fragment of unintegrated linear viral DNA, suggesting that these fragments were linked to cellular DNA sequences.

As digestion with *Eco*RI, *Pvu*II, or *Xba*I produced single viral DNA-containing fragments of unequal size in the two progenitor strains, we have used these enzymes to characterize the ecotropic MuLV DNA sequences in the 26 BXD RI strains. The results obtained by *Xba*I restriction enzyme analysis are shown in Fig. 2 and were confirmed by *Eco*RI and *Pvu*II digestion (data

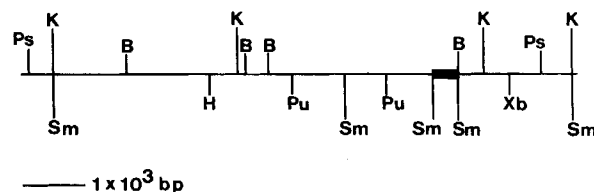


Fig. 1 Restriction endonuclease map of unintegrated linear AKR MuLV DNA²⁰. The unintegrated linear viral DNA, which is drawn 5' to 3' with respect to viral RNA, is 8.8 kb long and is not cleaved by *Eco*RI. The restriction sites of *Ps* (*Pst*I), *K* (*Kpn*I), *B* (*Bam*HI), *H* (*Hind*III), *Pu* (*Pvu*II), *Sm* (*Sma*I) and *Xb* (*Xba*I) are indicated. The 400-bp *Sma*I fragment, representing the ecotropic-specific sequences, is indicated by the solid block. These ecotropic-specific sequences have been subcloned into the *Eco*RI site of pBR322 and this plasmid was used here as a hybridization probe.

Table 1 Inheritance of the *d* allele and ecotropic MuLV DNA sequences in BXD and AKXD RI strains

Locus†	BXD RI strain*																									
	1	2	5	6	8	9	11	12	13	14	15	16	18	19	20	21	22	23	24	25	27	28	29	30	31	32
<i>d</i>	D	B	D	D	D	D	B	D	B	B	B	D	D	D	B	D	D	B	B	D	B	B	D	D	B	B
<i>Dbv</i>	D	B	D	D	D	D	B	D	B	B	B	D	D	D	B	D	D	B	B	D	B	B	D	D	B	B
<i>Blv</i>	D	B	D	D	D	D	D	D	B	B	D	D	D	B	D	D	D	B	B	D	D	B	D	B	D	D

Locus†	AKXD RI strain*																									
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	20	21	22	23	24	25	26	27
<i>d</i>	D	D	D	A	D	A	D	D	A	A	D	D	D	D	A	D	D	A	D	D	A	A	D	D	D	A
<i>Dbv</i>	D	D	D	A	D	A	D	D	A	A	D	D	D	D	A	D	D	A	D	D	A	A	D	D	D	A
<i>Akv-1</i>	A	D	A	A	A	A	D	A	D	A	D	A	D	A	A	D	A	D	A	D	A	A	A	D	D	D
<i>Akv-3</i>	D	A	D	A	A	A	D	A	A	D	A	A	A	A	D	D	D	D	D	D	A	D	A	D	A	A
<i>Akv-4</i>	A	A	D	A	D	A	A	D	A	D	D	A	A	A	D	D	D	A	D	D	D	A	A	D	A	A

* D, B and A were used to denote the alleles inherited from DBA/2J, C57BL/6J and AKR/J respectively.

† BXD and AKXD RI strains were initially classified with respect to the dilute (*d*) allele by visual inspection. Albino AKXD strains (5–7, 9, 10, 13, 15, 16, 18, 19, 21, 22 and 24) were classified with respect to the *d* allele by outcrossing a single male to DBA/2J females²⁴. The ecotropic MuLV loci of DBA/2J (*Dbv*), C57BL/6J (*Blv*) and AKR/J (*Akv-1*, *Akv-3* and *Akv-4*) were assigned from the data shown in Figs 2 and 3.

not shown). Eleven BXD RI strains inherited only the DBA/2J provirus, five strains inherited only the C57BL/6J provirus, three strains inherited both proviruses and seven strains inherited neither provirus. The segregation of the DBA/2J and C57BL/6J ecotropic proviruses (summarized in Table 1) was consistent with two independently segregating loci (the sites of the DBA/2J and C57BL/6J endogenous ecotropic proviruses are designated *Dbv* and *Blv* respectively).

A search for association of the endogenous ecotropic MuLV genome of these strains with one of more than 110 genetic markers distributed over 15 chromosomes, for which the BXD RI strains have been typed, revealed complete concordance between *Dbv* and the dilute (*d*) locus among all 26 RI strains. This places the *Dbv* locus near *d* on chromosome 9.

Characterization of ecotropic MuLV DNA sequences in AKXD RI strains

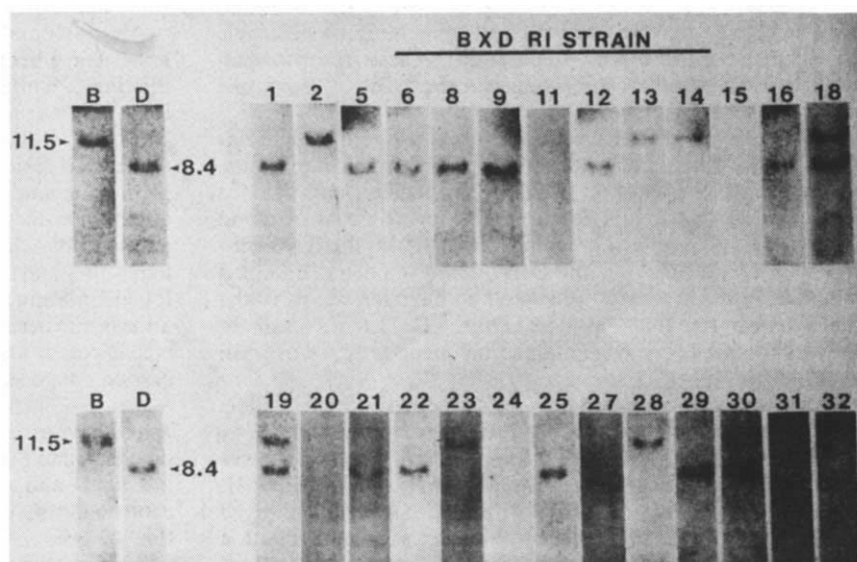
To confirm the chromosome 9 location of the DBA/2J ecotropic provirus, we examined by Southern blotting and hybridization with the ecotropic MuLV-specific probe DNAs of 27 AKXD RI strains derived from the cross of AKR/J and DBA/2J. DNA of all AKR/J mice contains three ecotropic proviruses, *Akv-1*, *Akv-3* and *Akv-4* (*Akv-4* has been previously referred to as *Akv-2J*; ref. 13). Digestion of AKR/J DNA with *PvuII* generates three viral DNA-containing junction fragments that correspond to *Akv-1* (4.8 kilobases (kb)), *Akv-3* (3.9 kb) and *Akv-4* (8.3 kb) (Fig. 3). The assignment of these three fragments to specific *Akv* loci was done by restriction enzyme analysis of DNAs from three AKXL RI strains that were derived from the cross of AKR/J and C57L/J. These three RI strains were

previously shown to contain only *Akv-1*, *Akv-3* or *Akv-4* following segregation of these loci during inbreeding of the AKXL RI strains (Fig. 3)¹³. In addition to the three viral DNA fragments normally found in AKR/J DNA, DNA from the two AKR/J mice used in these experiments also produced an additional 3.6-kb fragment (Fig. 3). This fragment seems to be less intense than the other viral DNA-containing fragments, suggesting that these DNA sequences are still segregating in AKR/J mice. This fragment was not detected in any of the AKXD RI strains (Fig. 3), indicating that it was not present in DNA of the progenitor AKR/J mice used to generate the AKXD RI strains.

Digestion of AKR/J DNA with enzymes that monitor internal virus structure (*Bam*HI, *Kpn*I or *Pst*I) again demonstrated that these endogenous MuLV DNA sequences were structurally indistinguishable from the exogenous prototype AKR MuLV DNA (data not shown). *PvuII* digestion of DBA/2J DNA produced a single 5.4-kb viral DNA-containing junction fragment (Fig. 3). As this fragment does not co-migrate with any of the fragments in *PvuII*-digested AKR/J DNA, we used this enzyme to follow the segregation of ecotropic MuLVs in DNA of the 27 AKXD RI strains (Fig. 3). The results of this analysis are summarized in Table 1. In addition to the one DBA and three AKR loci expected, we also found that five AKXD RI strains (AKXD-4, 11, 17, 24 and 25) have acquired additional ecotropic-specific DNA sequences detected as 5.3-, 7.2-, 3.4-, 6.7- and 5.9-kb DNA fragments respectively. The 5.3-kb fragment found in AKXD-4 DNA was shown not to be the DBA provirus by *Xba*I restriction enzyme digestion (data not shown).

The origin of these additional ecotropic sequences is unknown but they may result from germ-line reintegration of ecotropic MuLVs during inbreeding, a form of gene duplication that has

Fig. 2 Characterization of the endogenous ecotropic MuLV sequences in DNA of 26 BXD recombinant inbred and progenitor strains. High molecular weight DNAs were isolated from spleens of several mice of each of the progenitor strains and two mice of each of the 26 BXD RI strains. Spleens were disrupted in Dounce tissue homogenizers (in the presence of 0.25 mg ml⁻¹ pronase, 0.5% SDS, 0.15 M NaCl and 0.015 M sodium citrate), incubated at 37 °C for 30 min and high molecular weight DNAs were then extracted as previously described²¹. DNAs (10 µg per lane) were digested to completion with *Xba*I, electrophoresed through 0.6% agarose gels, transferred to nitrocellulose filters²¹ and hybridized²² in total volume of 7–12 ml with 5 × 10⁶ c.p.m. ml⁻¹ ³²P-labelled pBR322-containing ecotropic-specific viral DNA probe¹. The molecular weights (in kilobases) of the viral DNA-containing fragments detected in the progenitor (B, C57BL/6J; D, DBA/2J) and BXD RI (1, 2, 5, 6, 8, 9, 11–16, 18–25 and 27–32) strains were calculated using ³²P-labelled *Hind*III-digested λ DNA electrophoresed in parallel lanes of the same gels.



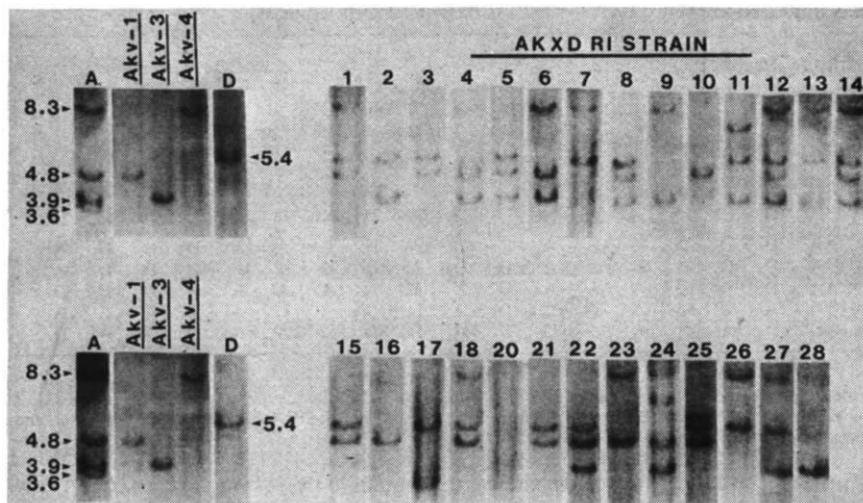


Fig. 3 Characterization of the endogenous ecotropic MuLV sequences in DNA of 27 AKXD recombinant inbred and progenitor strains. DNAs of AKR/J, DBA/2J and 27 AKXD RI (1–18, 20–28) strains were digested to completion with *PvuII* and analysed as described in Fig. 2 legend. In addition, DNAs of three AKXL (AKR/J × C57L/J) RI strains that have previously been shown to contain only *Akv-1* (AKXL-21), *Akv-3* (AKXL-28), or *Akv-4* (AKXL-13)¹³ were digested to completion with *PvuII*, analysed as described in Fig. 2 legend and used to identify the AKR/J proviruses inherited by the AKXD RI strains. Some of the fragments shown in this figure are light and difficult to see but were easily detected in the original autoradiograph. These fragments have been included in the data summarized in Table 1.

been shown to occur in congenic NIH *Akv-1* sublines of mice¹⁴. AKXD strains 11, 17 and 24 seem to carry the additional proviral DNA sequences in a homozygous state (determined by intensity of hybridization relative to other *Akv* loci in the same lane); AKXD-4 and 25 may be heterozygous for the additional ecotropic viral DNA sequences.

The segregation of ecotropic virus loci in the AKXD RI strains indicated that the AKR/J and DBA/2J ecotropic proviruses are unlinked (Table 1). In addition, the DBA/2J ecotropic provirus again segregated concordantly in 27/27 RI strains with the *d* allele, thus confirming the close linkage between these two genes. From the data presented in Figs 2 and 3, we can estimate that the 95% upper confidence limit of the recombination frequency between *Dbv* and *d* (assuming they are separable) is no greater than 0.015 (ref. 12).

Association of DBA/2J endogenous ecotropic MuLV with *d* allele in inbred and mutant mouse strains

DNA of five inbred and two mutant mouse strains, which carry the *d* allele, were examined by restriction enzyme analysis for their ecotropic MuLV DNA content. *PvuII* digestion of DNA prepared from spleens of five inbred strains, I/LnJ, P/J, BDP/J, SEA/GnJ and HRS/J, and two strains carrying chromosome 9 mutations, *cw* and *fd* (curly whiskers and fur deficient), all generated a 5.4-kb viral DNA-containing fragment characteristic of the DBA/2J ecotropic MuLV (Fig. 4). Further analysis with *EcoRI*, *XbaI*, and *HindIII* confirmed that each strain carried the DBA ecotropic provirus (data not shown). In addition, we have examined DNA prepared from 46 inbred strains and substrains that carry the wild-type allele at the *d* locus and have confirmed that they also do not carry the DBA ecotropic MuLV (N.A.J. *et al.*, in preparation). These results again confirm the close association between the DBA/2J ecotropic provirus and the *d* allele.

Integration of the DBA ecotropic provirus seems to be relatively stable. The dilute coat colour mutation is an old mutation of fancy mice. In 1909, C. C. Little produced a pair of mice homozygous for the coat colour alleles for *dilute*, *brown* and *nonagouti*, and began inbreeding the DBA strain. Two sub-strains of DBA, DBA/1J and DBA/2J, have been maintained independently for over 50 years and both strains still carry the same ecotropic MuLV as does DBA/2DeJ, a substrain of DBA/2J that has been maintained independently for over 30 years¹⁵ (Fig. 4).

All seven strains analysed in Fig. 4 contained both the *d* allele and DBA ecotropic MuLV, suggesting that these strains may have acquired the *d* allele from DBA mice, and this was confirmed by historical records for most strains. In 1926, W. H. Gates crossed Little's *dba* stock with L. C. Strong's pink-eyed dilution stock to generate a *brown*, *dilute*, *pink-eyed* dilution stock, that, when inbred, became the BDP/J strain¹⁶. The BDP

strain was one of the progenitor strains of P/J¹⁶. Similarly, SEA/GnJ is a recombinant inbred strain generated from BALB/c × P/J¹⁶ and this strain appears to have inherited the *d* allele and both DBA and BALB/c ecotropic MuLVs (the 4.3-kb fragment found in SEA/GnJ DNA has been shown by restriction enzyme analysis to be identical to the BALB/c ecotropic provirus; N.A.J. *et al.*, in preparation). Finally, the *cw* mutant strain acquired the *d* allele by a recent cross to DBA (H. Sweet, personal communication) and the *fd* mutation originally occurred in a DBA mouse¹⁵. The origin of the *d* allele of HRS/J and I/LnJ is unclear. HRS/J mice carry both ecotropic MuLVs that were detected in SEA/GnJ DNA. In addition to the DBA provirus, I/LnJ mice (a low leukaemic strain) also carry four other ecotropic proviruses and one of these proviruses is similar to the ecotropic MuLV of BALB/c.

Characterization of ecotropic MuLV DNA sequences in a spontaneous DBA/2J dilute revertant

We have also analysed DNA prepared from the *d*^{+2J} spontaneous revertant of DBA/2J¹⁵ to determine whether these mice still carry the endogenous DBA/2J ecotropic MuLV genome. *PvuII* digestion of DNA prepared from two mice homozygous for *d*^{+2J} (Fig. 5) showed that reversion of *d* to *d*⁺ was associated with concordant loss of the DBA/2J ecotropic-specific MuLV DNA sequences. This loss was confirmed by digestion of DNA from the revertant strain with enzymes that generate internal viral DNA-containing fragments (*Bam*HI, *Kpn*I, or *Pst*I) and virus-cell DNA junction fragments (*Xba*I) (data not shown).

Although multiple occurrences of *d*^l-like mutations (*dilute-lethal*) have been reported, as well as an allele that has a slight diluting (*d*^s) effect on coat colour, no mutations closely resembling the original *d* mutation have been reported. The gene product(s) of the *d* locus is unknown, but the diluted coat colour appears to result from the deposition of abnormally clumped pigment granules in the hair shaft¹⁷. To determine whether other dilute-like coat colour mutations were also associated with an ecotropic virus genome, we analysed DNA of the dilute-lethal (*d*^l) and ashen (*ash*) mutant mouse strains. The dilute-lethal mutation is a spontaneous chromosome 9 mutation at the *d* locus that occurred in a C57BL/Gr mouse and resulted in both a coat-colour and a behavioural change¹⁵. The *d*^l allele is now carried congenic in a linkage testing stock, DLS/Le, denoted *+/+* in Fig. 5. Homozygous *d*^l mice die at approximately 16–21 days of age (P. W. Lane, personal communication). *PvuII* digestion and Southern blotting of DNA prepared from 14-day old *d*^l/*d*^l and control (*+/+*) mice produced no detectable ecotropic MuLV DNA sequences (Fig. 5). In addition, restriction analysis of DNA from C3H/HeSn mice carrying the ashen (*ash*) mutation (a spontaneous dilute-like coat colour mutation

that occurred in the C3H/HeSn strain and is closely linked to the *d* locus) showed no detectable differences from control (+/+) mice (Fig. 5). These results indicate that the dilute-like coat colour mutations arising in these two strains are not causally related to the presence of ecotropic MuLV genomes.

Forward and reverse mutation rates at the *d* locus

In 1967, Schlager and Dickie¹⁸ reported the results of a study, based on the examination of 3.5 million inbred and F₁ hybrid mice, to determine the spontaneous mutation rates (both forward and reverse) at each of five specific coat-colour loci (nonagouti, *a*; brown, *b*; albino, *c*; dilute, *d*; and leaden, *ln*). No detectable differences in the forward mutation rates (8.9×10^{-6} mutations per locus per gamete) among the loci were detectable (after correction for a relatively small number of gametes tested at the *a* locus). Examination of back-mutation rates indicated that *a* and *d* loci back-mutate at a significantly higher rate than *b* and *c* loci (4.7×10^{-6} (*a*) and 3.3×10^{-6} (*d*) compared with $<0.6 \times 10^{-6}$ (*b* and *c*)). The number of gametes tested at the *ln* locus was too small to determine a back-mutation rate.

If the dilute mutation of DBA mice had originally been induced by virus integration, then reversion to wild-type coat colour may be the result of deletion of all or part of the virus genome. As the DBA ecotropic provirus is terminally redundant (determined by *Pst*I digestion; data not shown), analogous to prokaryotic and eukaryotic transposable elements, these viral DNA sequences may be lost by homologous recombination. A mutation which may be analogous to dilute was reported to occur in yeast by insertion of the transposable element *Ty1* into a 5'-noncoding region of the *his4* gene¹⁹. Reversion to a wild-type *his4* phenotype resulted from excision of most, but not all, of the transposable element.

Analysis of the DBA *d*^{+2J} revertant for ecotropic-specific viral DNA sequences did not allow us to determine whether all of the viral DNA sequences had been lost in the revertant strain because our hybridization probe recognizes only a 400-bp fragment from the *env* gene of AKR MuLV DNA. In future experiments we will determine the content of ecotropic MuLV DNA sequences present in the revertant strain and the mechanism by which these sequences were lost. In addition, Southern analysis of the *d*^{+2J} revertant DNA did not detect any new ecotropic virus-specific DNA sequences that might have been present in the revertant strain had the virus sequences reinserted in a new site. However, as the *d*^{+2J} revertant was initially maintained by repeated backcrosses to normal DBA/2J mice (E. S. Russell, personal communication), any reinserted

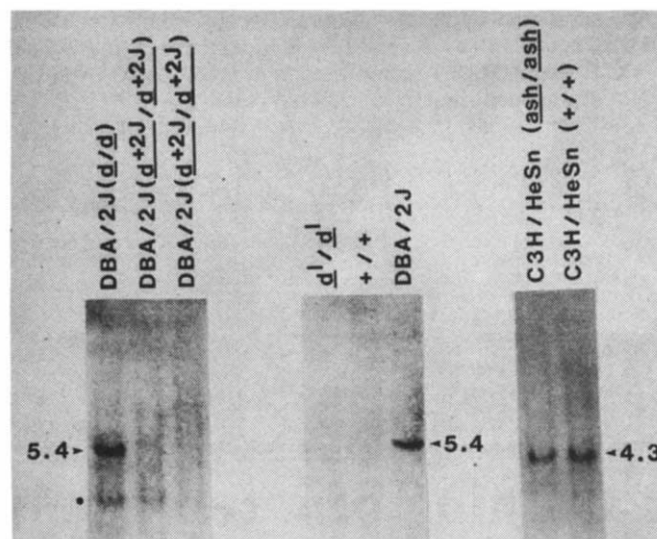


Fig. 5 Characterization of the endogenous ecotropic MuLV sequences in DNA of mouse strains carrying a forward or reverse mutation at the *d* locus. The dilute-lethal mutant (*d*¹/*d*¹) is homozygous for a forward mutation (*d*¹) at the *d* locus¹⁵ and the DBA revertant strain (DBA/2J *d*^{+2J}) represents a spontaneous reverse mutation (*d* → +) which is now inbred and homozygous for the revertant allele. The *ash* coat colour mutation, which spontaneously arose in C3H/HeSn mice, is on chromosome 9 linked to the *d* locus¹⁵. DNAs of mutant and control mice were digested to completion with *Pvu*II and analysed as described in Fig. 2 legend. The bands indicated by (●) do not represent hybridization of the probe to cellular DNA sequences (see Fig. 4 legend).

sequences not linked to the *d*^{+2J} revertant allele would have been lost.

In 1970, mice carrying the *d*^{+2J} revertant allele were made homozygous and have since been maintained by brother-sister inbreeding as a distinct subline of DBA/2 (E. M. Eicher, personal communication). It is conceivable that the deletion of the DBA/2J provirus occurred after, and hence independently, of the *d*^{+2J} reversion. This seems highly unlikely as the complete concordance of the DBA/2J provirus with the *d* mutation in many separately maintained inbred strains and 53 RI strains over many years indicates that such deletions are indeed rare. This possibility can best be tested by examining other *d* revertants.

During the original study of Schlager and Dickie¹⁸, four DBA/2J revertants of the *d* allele were identified and all were found to be allelic with the *d* locus. However, none of these reversions was maintained. Examination of the records currently kept at the Jackson Laboratory for new DBA/2J *d* allele revertants indicated that mice of this phenotype are still identified (one or two new mutations per year per 300,000 DBA mice). We are currently screening the Jackson Laboratory DBA mouse colony for new *d* allele revertants.

Finally, if the dilute mutation is the direct result of virus integration, the possibility exists that the integration of other ecotropic proviruses within the mouse genome may result in mutations as well. We are now analysing DNA of many mutant stocks for the presence of novel, integrated, ecotropic MuLV proviruses. Preliminary studies of this type have shown that at least one other coat colour mutation is closely associated with unique ecotropic MuLV DNA sequences.

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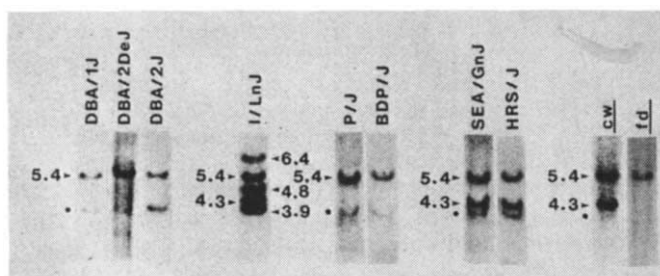


Fig. 4 Characterization of the endogenous ecotropic MuLV sequences in DNA of inbred and mutant strains carrying the *d* allele. DNAs of three DBA substrains (DBA/1J, DBA/2DeJ and DBA/2J), five inbred strains (I/LnJ, P/J, BDP/J, SEA/GnJ and HRS/J) and two mutant strains (*cw*, curly whiskers; *fd*, fur deficient) were digested to completion with *Pvu*II and analysed as described in Fig. 2 legend. The bands indicated by (●) do not represent hybridization of the probe to cellular DNA sequences as the molecular weight of the fragment (3.9 kb) is unaltered by digestion with various restriction enzymes and is also detected in lanes lacking DNA but containing tracking dye.

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Cloning of a representative genomic library of the human X chromosome after sorting by flow cytometry

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A library of 50,000 recombinants representative of the human X chromosome has been constructed. Human X chromosomes were physically separated using a fluorescence-activated cell sorter. The DNA was purified from the chromosomes, digested to completion with the restriction enzyme EcoRI and cloned into the phage λ gtWES. λ B. The X-derived nature of the recombinants was confirmed by hybridization to rodent/human cell line DNA containing only the human X chromosome. Such libraries will be particularly useful for the investigation of genetic diseases such as Duchenne muscular dystrophy, where the basic defect has not been elucidated, and of neoplasia, where several specific chromosomal anomalies, particularly for the leukaemias, have been identified.

MOST partial chromosome-specific libraries have so far been isolated using human-rodent hybrid cell lines¹⁻³. However, such procedures will only be applicable if the human chromosome of interest is stable in the hybrid cell. Even so, using this technique, only ~50% of the X-chromosomal DNA sequences can be identified, as there is considerable sequence homology between these sequences from mouse and human⁴. The use of chromosome-specific highly repetitive sequences, as applied to the Y chromosome^{5,6}, will only allow the isolation of a small proportion of the total chromosome-specific DNA, and will of necessity exclude clones containing coding sequences not adjacent to repetitive sequences. The cloning of the entire cellular genome to give a complete library (as, for example, for a 48,XXXX cell line⁷) allows the isolation of chromosome-specific probes, but a very large number of clones must be screened before a small number of specific sequences can be identified. Here we describe a more direct and simple approach for the cloning of genomic libraries containing sequences corresponding to a large proportion of any single human chromosome. The method involves sorting X chromosomes from a human cell line, in this case a 48,XXXX cell line, on a fluorescence-activated cell sorter (FACS-II).

Chromosome sorting

Metaphase chromosomes were prepared for flow cytometry by a previously described procedure which involves lysis of metaphase cells with digitonin in a buffer containing the polyamines spermine and spermidine⁸. DNA-based cytophotometry

has suggested that the human X chromosome has a DNA content intermediate between those of chromosomes 7 and 8 (ref. 9). Unfortunately, it is not possible to karyotype the chromosomes by conventional banding techniques after sorting because the chromosomes are too condensed. To identify the X chromosome unequivocally, flow karyotypes were obtained from the diploid fibroblast cell line MRC-5, 46,XY, and from the lymphoblastoid cell line GM1416, 48,XXXX (Human Genetic Mutant Cell Repository, New Jersey). The results are shown in Fig. 1a,b; there is clearly a peak in the region of chromosomes 7 and 8 which is larger in the flow karyotype from the GM1416 cells. Computer analysis of these data indicates that the area of this peak is four times greater in GM1416 cells than in MRC-5 cells and therefore may be attributed to the X chromosome. The assignment of the other human chromosomes to the peaks is based on a study which correlated centric heterochromatin polymorphisms with the corresponding flow karyotypes¹⁰. In fact, the differences between the flow karyotypes in Fig. 1a and b, other than that due to the X chromosome, are the result of such polymorphisms in the centric heterochromatin of certain chromosomes.

Chromosomes corresponding to the peak due to the X chromosome were sorted from within the window shown in Fig. 1b and re-analysed on the FACS-II. The fluorescence profile (Fig. 2) demonstrates the purity of the peak obtained from the initial sorting, as there is little contamination with chromosomes from outside the sorting window. The bulk of this sorted fraction will correspond to the X chromosome, with some contamination from chromosomes 7 and 8. Computer analysis

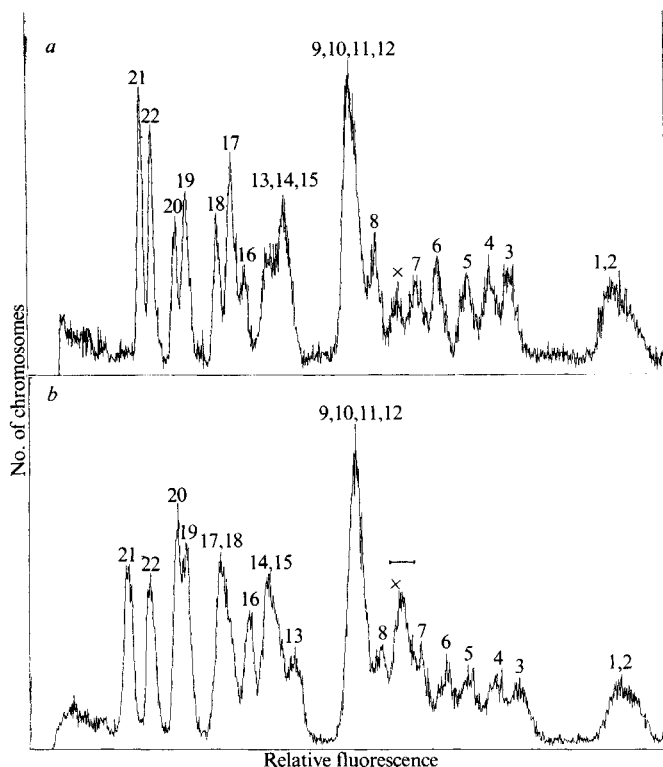


Fig. 1 Flow karyotypes from: *a*, MRC-5, 46,XY diploid fibroblast cells; *b*, GM1416, 48,XXXX lymphoblastoid cells. These results were obtained from metaphase chromosomes prepared by the protocol described in ref. 8. Chromosomes were stained with ethidium bromide ($100 \mu\text{g ml}^{-1}$) and excited with a Spectra-Physics 164-05 laser producing an output of 1.5 W at 488 nm. The fluorescence was detected through a 550-nm filter and measured with an EMI photomultiplier tube. The positions of the human chromosomes giving rise to each peak are indicated and the horizontal bar indicates the sorting window used for the purification of the X chromosome. The data collected on the FACS-II were transferred to a Wang 2200S computer for analysis and are presented as relative quantities.

of the flow karyotype in Fig. 1*b* suggests that this level of contamination may be ~10%.

Cloning of sorted X-chromosomal DNA

For the construction of the library, 2×10^6 copies of the X chromosome were sorted (400–500 ng DNA; sorted at 1,000 chromosomes per min) and the sorted DNA was treated with proteinase K ($50 \mu\text{g ml}^{-1}$) and extracted with phenol-chloroform. The addition of tRNA was essential for the ethanol precipitation but because it was found to inhibit the *in vitro* packaging, a minimum quantity was added ($10 \mu\text{g ml}^{-1}$). The addition of large amounts of tRNA also inhibited the action of *EcoRI* restriction endonuclease. The sorted DNA was digested extensively with *EcoRI* and ligated to the arms of the vector λgtWES . λB (ref. 11). The ligated samples were packaged *in vitro* with a packaging efficiency of 10^8 plaque-forming units (PFU) per μg DNA¹². The cloning efficiency was typically 10^5 PFU per μg of cloned insert. A total of 50,000 recombinants were obtained, containing a background of parental phage of <5%, as assayed by ligating and packaging the parental phage arms alone. To date, 90 recombinants have been picked at random from the library and screened by hybridization with nick-translated total human DNA. All of them give a positive hybridization signal and therefore contain a human DNA sequence insert, confirming that the background in the library due to parental phage is low. The average size of inserts is 7 kilobases (kb) and clones have been identified with inserts in the range of 4–14 kb. Fragments of less than 2 kb or larger than 14 kb ligated into this vector do not package into viable phage¹³;

however, these will represent only a very small proportion of the X chromosome.

Identification of X-chromosome clones

A large proportion of the human genome contains repetitive sequences¹⁴. Pre-screening of the total X library with nick-translated human DNA¹⁵ indicated that ~70% of the recombinants contained sequences repeated 150 times or more in the genome. Clones that did not show a hybridization signal in this assay were selected for further analysis in the hope of obtaining single-copy X-specific probes for use in the construction of a genetic linkage map of the X chromosome using restriction enzyme site polymorphisms.

The low-copy number human X-chromosome clones selected were characterized by hybridization of the purified recombinant DNA to human female DNA, DNA derived from a human-mouse hybrid cell line containing only the human X chromosome (HORL9.X42.2 (ref. 16), hereafter referred to as HORL9.X) and total mouse genomic DNA digested to completion with *EcoRI* using the Southern blot technique¹⁷. Figure 3 shows three clones containing sequences which hybridize to fragments in total human DNA and HORL9.X DNA but not in mouse DNA. The presence of other copies of these sequences on autosomes cannot be excluded here. However, the hybridization times and autoradiography exposures for the majority of clones analysed, which give simple band patterns in a Southern blot, are compatible with the presence of a single copy of the sequence per haploid genome. X-specific clones are being identified using gene dosage dot hybridization with DNA from the 48,XXXX cell line and male and female blood. These recombinants are now being used to detect associated restriction enzyme site polymorphisms. The size of the DNA fragments hybridizing in the Southern blots are identical to the size of the human inserts in the phage in each case, demonstrating that no DNA rearrangements have occurred during the cloning procedures. The hybridization signal in the HORL9.X DNA track was often less intense than that observed in the human DNA track although the total amount of DNA blotted in each case was identical. This is probably explained by the presence in the HORL9.X cell line of only 1 human X chromosome and 60 mouse chromosomes (P. Goodfellow, personal communication), compared with 2 X chromosomes and 1 pair of the 22 autosomes in the human female reference DNA.

Several clones contain fragments which hybridize to repetitive sequences in the mouse genome even after high-stringency washing of blots ($0.1 \times \text{SSC}$, 65°C), indicating a high degree of conservation of some repetitive DNA during evolution. Figure 4 shows an example of two such clones, hybridizing to both mouse and human repetitive sequences. In Fig. 4*b* the human track

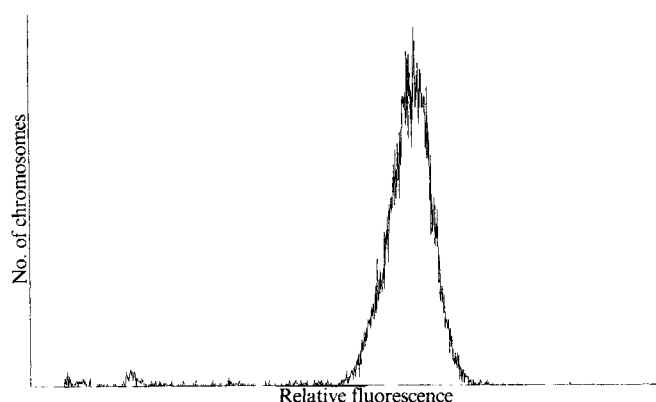


Fig. 2 The flow analysis of chromosomes sorted in the window indicated in Fig. 1*b*. The chromosomes were restained with ethidium bromide and analysed in the same conditions as for Fig. 1. This fraction was used for the construction of the phage DNA library.

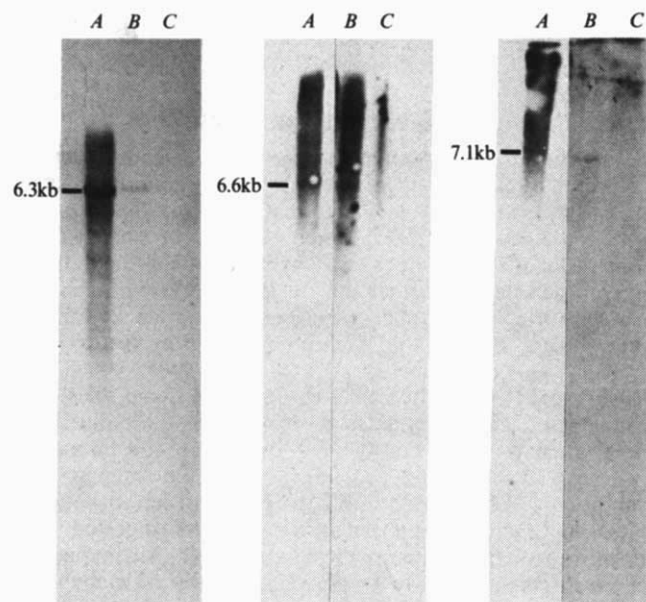


Fig. 3 The pattern of hybridization of recombinant phage DNA to DNA from human female blood (A), HORL9.X cell line (B) and mouse DNA (C). DNA (10 µg) was digested with *EcoRI*, electrophoresed through agarose and transferred to nitrocellulose¹⁷. Phage DNA were prepared¹⁹ and nick-translated to a specific activity of 10^8 c.p.m. per µg (ref. 22). Blots were hybridized at 65 °C for 30 h in $3\times$ SSC, 0.1% SDS, $100\text{ }\mu\text{g ml}^{-1}$ denatured herring sperm DNA, $50\text{ }\mu\text{g ml}^{-1}$ poly(A). (SSC is 0.15 M NaCl, 0.015 M Na citrate, pH 7.0.) After hybridization, blots were washed with $3\times$ SSC, 0.1% SDS at 65 °C and finally with $0.1\times$ SSC, 0.1% SDS also at 65 °C (refs 20, 21). Autoradiography was at -70 °C for 18 h for track A and 36 h for tracks B and C. All manipulations of recombinants were carried out in CII containment conditions in accordance with the GMAG guidelines for recombinant DNA research.

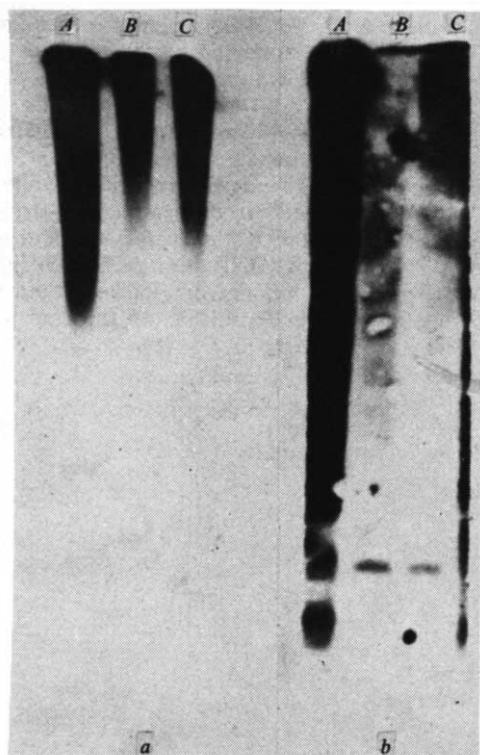


Fig. 4 The pattern of hybridization of recombinant phage to DNA from human female blood (A), HORL9.X cell line (B) and mouse (C). The conditions of hybridization are detailed in Fig. 3 legend. Autoradiography was carried out at -70 °C for 18 h. For explanation of a and b, see the text.

shows extensive hybridization with repetitive sequences not evident in the HORL9.X or mouse DNA which presumably must be present on human autosomes as well as the X chromosome. The single band seen in the human, HORL9.X and mouse DNA tracks in Fig. 4b and the repeats present in all tracks in Fig. 4a may not, of course, be present on the X chromosome. This is now being investigated by gene dosage using DNA isolated from the GM1416 cell line (48, XXXX), and from female and male blood.

Only 1 clone out of 30 so far characterized with the HORL9.X cell line gave no signal with either HORL9.X or mouse DNA. This sequence may be derived from human chromosomes 7 or 8 or a portion of the X chromosome absent from the cell line, and indicates the purity of the sorted X-chromosome peak from the FACS-II sorter. Twenty-seven clones hybridized to HORL9.X and human DNA but not to mouse DNA and six of these hybridized as single copy sequences. Two clones hybridized as a smear to HORL9.X, human and mouse DNA and therefore could not be assigned to the X chromosome by this technique.

Closing remarks

A library of 50,000 recombinants has been constructed from X chromosomes sorted on a FACS-II cell sorter. In view of the high purity of the sorted chromosomes and the average size of the cloned DNA fragments, this library contains sufficient sequences to ensure that the most of the X chromosome is represented¹⁸. Sequences giving either single bands or smears in Southern blot analyses have been identified. Some repetitive sequences may be X specific and many show homology to sequences on the X chromosome as well as autosomes. Several fragments show a high degree of homology with mouse genomic repetitive sequences. X-chromosome clones from this library which give single bands in Southern blots have been characterized and are now being used to define DNA sequence polymorphisms suitable for the genetic analysis of sex-linked diseases such as Duchenne muscular dystrophy. The extension of this technique to the cloning of gene libraries of each human autosome does not require the preparation of human-rodent cell lines and should become easier with the use of different fluorescent dyes for sorting. Autosome-specific libraries will thus be available to study genetic diseases and chromosomal rearrangements in all human chromosomes.

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LETTERS TO NATURE

The nearby interstellar medium

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The high dispersion spectrometer on board the International Ultraviolet Explorer (IUE) satellite was used to observe interstellar absorption lines in Rasalhague (α Oph). This star is located 18 pc from the Sun in the direction of the North Polar Spur, a prominent radio continuum feature in the Ophiuchus region of the sky. The satellite results, combined with previous interstellar line data and observations of the 'local interstellar wind' and soft X-ray emission, support earlier suggestions that the Sun is immersed in a supernova remnant which may be an extension of the 'Loop I' or 'North Polar Spur' supernova remnant seen in the Scorpius-Ophiuchus region of the sky.

Rasalhague is a bright unreddened A-type star (A5 III, $V = 2.1$ mag) which has interstellar lines sharply defined against stellar absorption lines because the star rotates rapidly ($v \sin i = 218 \text{ km s}^{-1}$). The interstellar lines are also offset from the stellar lines with respective central velocities of -26 and $+13 \text{ km s}^{-1}$. The interstellar line velocity near -26 km s^{-1} (all velocities are heliocentric unless otherwise indicated) is well determined by high resolution optical observations which show only one neutral interstellar cloud in front of Rasalhague^{1,2}. It has been pointed out previously³ that this cloud has about the same velocity as the 'local interstellar wind' (see below), and therefore must be local. It will be referred to here as the 'local gas'.

Four long wavelength (LWR) and three short wavelength (SWP) high dispersion IUE exposures were made of this star. The individual exposures were numerically summed after they had been checked for defects, unrecognized réseaux marks and so on in the vicinity of the individual lines, and the equivalent widths were measured with a planimeter from the stacks (see Table 1).

Column densities were calculated from the equivalent widths using the Stromgren curve of growth, which relates the optical depth in the absorption line to the number of absorbing particles as a function of the Doppler constant b , and are listed in Table 1⁴. All lines were assumed to be broadened intrinsically by $b = 4\text{--}5 \text{ km s}^{-1}$. The Fe^+ data do not permit a b value significantly outside this range as the weakest Fe^+ absorption lines fall on the unsaturated shoulder of the curve of growth. The strong Mg^+ lines fall on the saturated portion of the curve of growth, however, resulting in large errors on these column densities.

The local gas has been observed in front of many of the more distant stars in Scorpius and Ophiuchus and, in particular, in front of ζ Oph and χ Oph which are 180 pc from the Sun^{3,5}. High resolution optical and UV observations have been made to separate this weak local feature from the stronger background feature near -15 km s^{-1} in the spectra of these two stars. Column densities of six ions in the local gas in the directions of the stars α Oph, ζ Oph and χ Oph, as found from these observations, are listed in Table 2, as well as the amount of neutral hydrogen associated with the local gas in these directions. The amount of neutral hydrogen in front of α Oph was derived from an integration of the 21-cm emission found between the velocities of -23 and -33 km s^{-1} in this direction¹. This column density is listed as an upper limit as part of that emission may come from behind this nearby star.

An estimate can be made of the average density for the interstellar material in front of α Oph. Assuming that the iron abundance in front of α Oph lies between the range of values

defined by ζ Oph and χ Oph, the column densities in Table 2 indicate an average hydrogen density in that direction of $n = 0.08 \pm 0.05 \text{ cm}^{-3}$. This estimate in the direction of α Oph agrees well with the interstellar density range of $n = 0.1\text{--}0.15 \text{ cm}^{-3}$ found from $\text{L}\alpha$ absorption studies of three stars within 3 pc of the Sun at several locations throughout the sky⁶. Furthermore, this similarity in densities for these nearest stars and α Oph, at 18 pc, suggests that the interstellar material in front of α Oph has a relatively uniform density distribution. These observations of the nearest stars also provide an estimate of the temperature of the local gas. The velocity dispersion parameter giving the best fit to these $\text{L}\alpha$ absorption lines corresponds to temperatures of $9 \pm 4 \times 10^3 \text{ K}$, indicating that the gas must be warm⁶.

The column densities listed in Table 2 were converted to abundances by normalizing to the neutral hydrogen column densities. These abundances are listed in Table 3, along with the cosmic abundances of the parent elements. They are close to the values expected for cosmic abundances, in contrast to the same ratios found in a 'typical' diffuse cloud (also listed). (The 'typical' ratios are based on the values found in the background stellar cloud in the direction of ζ Oph⁷.)

This variation between the abundances in the local cloud and a typical cloud is probably not an artefact of unknown ionization conditions. Uncertainties in the ionization could affect these ratios in two ways: either substantial quantities of the trace elements could be present in ionization states not listed in Table 3, or most of the hydrogen could be ionized. The former possibility can be definitely excluded because, except for Ca^+ , the elements listed in Table 3 represent the dominant ionization states in neutral interstellar gas where photons of energies greater than 13.6 eV are excluded. Also, the data in the direction of ζ Oph exclude more than 10% of the iron in the line of sight from being in alternate ionized states, and the Fe^{2+} that is present is located in the H II region around the star⁸. The second possibility, that large amounts of ionized hydrogen in the line of sight cause distorted abundances, also does not seem to be likely. Data on the interstellar gas nearest the Sun indicate that it could be up to 50% ionized (see below), but even with 50% ionization the local gas iron abundance, for example, does not approach the 'typical' cloud abundance. Therefore, the abundance anomalies in the local gas must be real.

What is the cause of these abundance anomalies? All the metals discussed above are mildly refractory elements, that is, they have condensation temperatures generally larger than 1,000 K. One of the initial results of the Copernicus satellite showed that the refractory elements have a widespread underabundance, typically by orders of magnitude, with respect to cosmic abundances in many diffuse interstellar clouds⁹. The explanation given for this underabundance is that these missing

Table 1 IUE observations of Rasalhague

Element	λ_{Lab} (Å)	Oscillator strength	Equivalent width (mÅ)	No. of exposures*	$\log N$ (cm^{-2})†
Fe^+	2,599.395	0.203	143 ± 24	3	13.78 ± 0.15
	2,585.876	0.0573	81 ± 17	3	
	2,382.034	0.328	155 ± 50	3	
	2,343.495	0.108	122 ± 24	3	
Mg^0	2,852.127	1.90	<40	3	<11.47
Mg^+	2,802.704	0.295	245 ± 80	3	15.01 ± 0.73
	2,795.528	0.592	208 ± 24	3	
Mn^+	2,576.107	0.288	<50	3	<12.5
Al^{2+}	1,854.7	0.539	≤ 26	1	<12.20

* The numbers of exposures averaged into the final stacks of each line.

† All column densities were calculated by fitting the available absorption lines to curves of growth with $b = 4\text{--}5 \text{ km s}^{-1}$.

Table 2 Column densities in local cloud

Ion	α Oph*	log N (cm ⁻²)	
		ζ Oph†	χ Oph‡
H ⁺	≤ 19.56 §	18.88	19.30
Na ⁺	10.48 (ref. 1)	11.20 (ref. 1)	≤ 10.93
Mg ⁺	15.01	14.57	—
Ca ⁺	11.18	10.85	11.54
Ti ⁺	11.22 (ref. 2)	11.48 (ref. 2)	11.8
Mn ⁺	< 12.5	12.53	—
Fe ⁺	13.78	14.21	14.00

Column densities shown are those of clouds with velocities -25 , -27 to -30 and -26 km s⁻¹, for α Oph, ζ Oph and χ Oph, respectively.

* See Table 1. † Ref. 7. ‡ Ref. 13. § See text.

elements are locked up in the interstellar dust grains found throughout space. However, there is a class of low mass clouds, known as 'high-velocity clouds', which share this property of possessing nearly cosmic abundances of refractory metals¹⁰. The abundance ratios in Table 3 establish that the local gas has high-velocity cloud-type abundances. High-velocity cloud abundances are believed to be caused by the disruption of the interstellar grains in a shock front, the amount of disruption depending both on grain material and shock velocity¹¹. For example, interstellar grains composed of ice, silicates or iron can be mostly destroyed, with the grain material returned to the gas phase, by the passage of shock fronts with velocities of < 100 km s⁻¹, while graphite grains are partially destroyed at these velocities¹². The low velocities observed for the local gas (10 – 20 km s⁻¹ in the local standard of rest) indicate that the local interstellar gas has slowed substantially since the passage of the shock front, but the unusually high gas-phase abundances remain.

Independent geometrical evidence supports the idea that the local interstellar medium may have been processed through a shock front. It has been suggested, because of the location of α Oph in the direction of the North Polar Spur, that the Loop I supernova remnant has processed the interstellar material near the Solar System¹³. The centre of this supernova remnant lies near the galactic coordinates $l = 330^\circ$ and $b = +18^\circ$, and has a diameter of $\sim 115^\circ$ with the centre located ~ 130 pc away^{14,15}. This would place the original explosion somewhere in the near side of the Scorpius–Centaurus association, and the remnant would have encountered a density gradient as it expanded. It would have expanded slowly and for relatively short distances in the directions away from the Sun as it encountered the large amount of interstellar material of the Scorpius–Centaurus association. However, it would have expanded rapidly and to greater distances in directions towards the Sun where it would have encountered little resistance from interstellar material. Thus we see that the Loop I remnant possesses the correct geometry to have enveloped the Sun.

There are two other entirely different sets of data available on interstellar material near the Sun. The first set are extensive observations of the 'local interstellar wind' which show that the neutral material immediately around the Sun is warm and partly ionized. The second set comprise the sky maps of soft X-ray emission which show a widespread million degree plasma around the Sun such as expected for the interior of a supernova remnant. These additional data are discussed below.

The term 'local interstellar wind' refers to the neutral interstellar gas which flows into the Solar System from an apparent direction rather near the solar apex. This material has been mapped throughout the sky in hydrogen Ly α and He 584 Å emission, where the emission results from solar line radiation which has resonantly scattered off the neutral material (which might penetrate, typically, to within a few AU of the Sun before becoming ionized). Theoretical interpretations of these maps indicate that the local interstellar gas, before interacting with the Solar System, can best be described by a hydrogen density of $n_H = 0.08 \pm 0.02$ cm⁻³, a helium density of $n_{He} =$

0.013 ± 0.03 cm⁻³, and a temperature of $T = 10 \pm 5 \times 10^3$ K. The hydrogen gas may be partially ionized if the H/He ratio is cosmic, implying that the total particle density outside the Solar System is $n_0 = 0.15 \pm 0.03$ cm⁻³. The best velocity vector for the local wind indicates that it has a heliocentric velocity of 25 ± 5 km s⁻¹, in the apparent upwind direction of α (1950) $\approx 225^\circ$ and δ (1950) $\approx -16^\circ$ (refs 16–22).

The local gas described here has a velocity that corresponds to the material which produces the local interstellar wind³. The local wind velocity vector projected in the directions of α Oph, ζ Oph and χ Oph yields velocities of -22 , -25 and -25 ± 5 km s⁻¹, respectively, which compare well with the velocities found from the Na⁺ optical absorption lines, which are -25 , -27 to -30 and -26 km s⁻¹, respectively¹. The star α Oph, which is the furthest from the upwind direction ($> 20^\circ$ away), shows a velocity in the optical absorption lines slightly below the projected local wind velocity vector. This difference may reflect internal motions within the local gas. Also, the average density in front of α Oph, $n = 0.08 \pm 0.05$ cm⁻³, is consistent with the local wind value, and again shows that the local neutral material is at most mildly clumped near the Sun in this direction.

The detailed physical state of the local neutral gas is fixed by these local wind observations as a low density, warm, and possibly partly ionized material, corresponding to the warm neutral material predicted to form where the properties of interstellar gas are dominated by violent supernova activity¹⁸. In the McKee and Ostriker interstellar medium model²³ the warm neutral gas has the approximate parameters $n \approx 0.4$ cm⁻³, $T \approx 8,000$ K and is about 15% ionized, with the temperature and ionization maintained by soft X-ray photons. These conditions correspond reasonably well with those found for the local neutral gas. It is shown below that there is observational evidence for the nearby hot plasma required as a source of the soft X-ray photons.

Recent soft X-ray sky surveys have found that low-energy X-rays (energy ≤ 0.3 keV) are emitted by a million degree plasma which surrounds the Sun, and which has in front of it no more than 5×10^{19} cm⁻² column density of neutral material²⁴. However, the local wind observations discussed above indicate that this million degree plasma is not the closest material to the Sun. On the basis of the velocity information discussed here, we see that the α Oph cloud is related to the local wind material; this view is then consistent with the X-ray observations as the neutral hydrogen column densities in the local gas all seem to be under this 5×10^{19} cm⁻² limit set by the soft X-ray data (see Table 2). The million degree plasma which explains the soft X-ray emission also accounts for the shock front which was invoked above to explain the unusual depletions in the local gas. Furthermore, it provides the necessary source of soft X rays to maintain the warm neutral gas. On the basis of these X-ray data, Sanders *et al.*²⁵ and Davelaar *et al.*²⁶ have suggested that the Sun is immersed in a separate local supernova remnant. A simpler explanation is that the Loop I supernova remnant has expanded to encompass the Solar System.

Table 3 Abundances with respect to hydrogen

Ion	α Oph	Local gas cloud*		Cosmic abundance†	Typical abundance‡
		ζ Oph	χ Oph		
Mg ⁺	$> 2.8 \times 10^{-5}$	4.9×10^{-5}	—	4.0×10^{-5}	1.3×10^{-6}
Ca ⁺	$> 4.2 \times 10^{-9}$	9.3×10^{-9}	1.7×10^{-8}	2.8×10^{-6}	1.9×10^{-10}
Ti ⁺	$> 4.6 \times 10^{-9}$	2.5×10^{-8}	3.2×10^{-8}	8.8×10^{-8}	3.7×10^{-11}
Mn ⁺	—	4.5×10^{-7}	—	3.4×10^{-7}	1.0×10^{-8}
Fe ⁺	$> 1.7 \times 10^{-6}$	2.1×10^{-5}	5.0×10^{-6}	3.4×10^{-5}	4.5×10^{-8}

* This corresponds to the clouds with velocities -25 , -27 to -30 and -26 km s⁻¹ for α Oph, ζ Oph and χ Oph, respectively.

† These values are the cosmic abundance of the parent elements, with respect to hydrogen³¹.

‡ The prototype 'typical' diffuse cloud is taken to be the main background cloud in front of ζ Oph¹.

From the various data, an overall picture of the interstellar gas near the Sun emerges. The optical and UV data summarized above confirm that the nearest 18 pc of interstellar material in the direction of the Ophiuchus constellation has a composition of refractory elements more like the cosmic abundance of these elements than the high depletions ordinarily found in diffuse clouds. This type of composition, common to a cloud class known as 'high-velocity' clouds, can be explained by the processing of interstellar material through a shock front such as that associated with a supernova remnant. However, the low velocity of the material indicates that this supernova remnant has slowed down since the passage of the shock front.

These data have been interpreted in the context of the McKee and Ostriker model of the interstellar medium. An alternative interpretation is that density-dependent accretion of gas phase species onto grains, instead of shock-front activity, controls the depletions in this low-density and low-velocity cloud. The existence of such a process has been inferred from observed correlations between the depletions of Ti^+ and Fe^+ and the average gas density in a line of sight^{27,28}. The relative geometries of Rasalhague and the Loop I remnant, combined with the X-ray data, provide strong evidence for an ambient supernova remnant and support the shock front interpretation. However, the slow rate of accretion in low-density clouds explains why the elements have not yet recondensed on to the surface of the grains in this relatively evolved supernova remnant.

Observations of the local interstellar wind as well as stars within 3 pc of the Sun show that this gas is warm ($T \approx 10,000\text{K}$) and may be up to 50% ionized. The local gas also has a density $n \approx 0.1\text{ cm}^{-3}$, and may either be mildly clumped near the Sun or distributed uniformly in front of $\alpha\text{ Oph}$. The soft X-ray data imply that the Sun is surrounded by a million degree plasma, but the local wind observations establish that this plasma must be behind the local neutral material. The total column density of neutral material associated with the local gas ($\approx 10^{19}\text{ cm}^{-2}$) is too low to shadow the soft X-rays, and is consistent with the X-ray results that $<5 \times 10^{19}\text{ cm}^{-2}$ of neutral material is between the hot plasma and the Sun.

These data form a consistent picture in which the Sun is immersed in the near-side of the Loop I supernova remnant. The shock front which processed the local gas would have been associated with the outer envelope of this remnant, and would have passed the Solar System some time in the past. The supernova remnant would have expanded asymmetrically because the parent star exploded on the near side of the Scorpius-Centaurus association. What now remains surrounding the Sun is the hot plasma that is interior to this remnant, and intermixed in some way with warm neutral material, some of which now immediately surrounds the Sun, and yields a high-velocity cloud interstellar spectrum which we see projected against the background stars in this region.

Combining the several sets of data suggests that this remnant has an age of $\sim 2\text{ Myr}$. Sanders *et al.*²⁵, Davelaar *et al.*²⁶ and Iwan²⁹ interpret the X-ray data as indicating a Loop I remnant age of $\approx 2 \times 10^6\text{ Myr}$. On the other hand, the Be^{10} concentration in deep sea sediments has been constant to within $\pm 30\%$ during the last 2 Myr, indicating that there has been no strong cosmic ray production near the Sun within that period³⁰. If both of these age limits are correct, then the Loop I remnant must be of the order of 2 Myr old.

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Note added in proof: Additional data and arguments for the occurrence of both ionized and neutral interstellar material near or around the Solar System have recently been presented (see refs 32–35).

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Evidence for an X-ray period in the 'Sco-like' source 2S0614+091

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A class of galactic X-ray source exists whose members show properties which have many similarities to those of Sco X-1, the brightest source in the X-ray sky^{1–3}. The X-ray emission from such sources is widely believed to arise from the transfer of matter on to a compact object in a close binary system, but despite extensive observations in many regions of the electromagnetic spectrum there has been little direct evidence to support this view. A study of the temporal behaviour of these Sco-like sources, using data from the Ariel V Sky Survey Instrument (SSI) is in progress; we report here the results of a detailed analysis for one such source, 2S0614+091, which undergoes significant variations in its X-ray intensity consistent with a period of 5.2 ± 0.3 days. This is the first discovery of a periodic modulation in the X-ray emission from this class of source and, despite problems regarding the mass-transfer mechanism, provides evidence for their binary nature. Our result has wider implications if 2S0614+091 is confirmed to be an X-ray burster.

The X-ray emission from 2S0614+091 is highly variable; measurements by the Uhuru⁴, Copernicus⁵, OSO 8 (ref. 6), SAS 3 (ref. 7) and Ariel V¹ satellites have indicated a general level of variability of between ~ 5 and 30 SSI counts per second (c.p.s.) ($1\text{ SSI c.p.s.} \equiv 5.1 \times 10^{-11}\text{ erg cm}^{-2}\text{ s}^{-1}$ averaged over 2–10 keV for a Crab-like spectrum). However, any characteristic time scales of variability have remained largely undefined; indeed, quiescent periods of X-ray emission, lasting for up to ~ 8 days, have also been noted^{1,5}. 2S0614+091 has been identified^{5,8} with a faint ($B \sim 18.8$ mag) blue star showing an UV excess and an emission feature associated with the C III/N III blend ($\lambda\lambda 4,640\text{--}4,650$) superimposed on an otherwise flat continuum. The optical magnitude has also been shown to be variable⁸. On the basis of its optical appearance and correlated X-ray intensity/spectral hardness variations, 2S0614+091 has been included as a member of the class of X-ray source with

Table 1 SSI observations of 2S0614+091

Data set	Modified Julian + calendar dates of observation		Period (days)	Comments
1	42,402–42,414	December 1974	—	Quiescent period of X-ray emission. Mean flux ~ 15 c.p.s. No evidence of variability.
2	42,488–42,502	March 1975	—	Mean flux ~ 20 c.p.s. Marginal variability and wide peak in χ^2 distribution centred on ~ 4.6 days.
3	43,093–43,115	November 1976	5.5 ± 0.6	Flux strongly modulated; see Fig. 2.
4	43,397–43,421	September 1977	5.0 ± 0.6	Flux strongly modulated; see Fig. 2.
5	44,091–44,103	August 1979	—	Quiescent period of X-ray emission. Mean flux ~ 10 c.p.s. No evidence of variability.
6	44,129–44,151	September 1979	5.1 ± 0.4	Flux strongly modulated; see Fig. 2.

properties similar to those of Sco X-1. This class also includes 2S1702–363 (GX349+2; Sco X-2), 2S1728–169 (GX9+9), 2S1735–444, 2S1758–250 (GX5–1) and 2S1813–140 (GX17+2) and as yet^{9,10} no significant periodic modulation in the X-ray flux from these sources, which would substantially support their membership of close binary systems, has been reported.

The present X-ray observations were made with the high energy (2–18 keV) detector system of the Leicester University SSI on board Ariel V between November 1974 and February 1980. Details of the instrument and data reduction techniques have been published elsewhere^{11,12}. The X-ray light curve of 2S0614+091 is shown in Fig. 1; the source is clearly variable. To quantify this variability and to investigate the possibility of periodic behaviour we have examined those data sets for which there was sufficient duration and sensitivity for a search for periodic modulation of the X-ray flux to be made. The acceptable data sets, listed in Table 1, consisted of data spanning typically 20 days. These were examined with maximum time resolution by subdividing the data into single-orbit fluxes (of duration ~ 90 min). For each data set these fluxes were folded into 10 phase bins over a series of trial periods in the range 0.5–8.0 days (these limits were chosen bearing in mind the SSI time resolution and lengths of the data sets) and for each trial period a simple χ^2 test was applied against the hypothesis that the binned data did not differ significantly from the mean value for that set. To support our belief that any periodicities indicated by the χ^2 tests were in fact real and not due to any peculiarities in our data sampling, a series of data were simulated having identical temporal coverage but with fluxes which were distributed randomly with a gaussian envelope about the mean source flux. These simulated data sets were folded in the same manner and identical χ^2 tests were performed.

For three of the six acceptable data sets for 2S0614+091 the results of the χ^2 tests indicated a significant flux modulation. The χ^2 versus period distributions for these sets are shown in Fig. 2; in each case a significant peak occurs in the range 5.0–5.5 days. The corresponding X-ray light curves are also presented in Fig. 2 and although they are masked occasionally by random variability on shorter time scales, show clear evidence for a periodicity in the above range. The remaining data sets showed no evidence for any significant modulation of the X-ray flux; the corresponding light curves indicated that the X-ray emission during these observations was at a low level of ~ 15 SSI c.p.s., consistent with the previously reported quiescent periods of X-ray emission from 2S0614+091 (refs 1, 5). However, because of their lower intensity and reduced statistical significance, the detection of a modulation at the same level in the X-ray flux during these stretches of data would be difficult to make. No peaks in the resulting χ^2 versus period distributions were found for the simulated data sets, the χ^2 values being significantly lower than for the actual data of sets 3, 4 and 6. Although no formal probability of the source modulation at the detected period can be made because of the contribution to χ^2 values from chaotic variability in the source, the tests on the simulated data show quite clearly that the χ^2 peaks found in the 2S0614+091 data cannot have arisen by chance nor can they be an artefact of the method of analysis or the observation sampling

intervals. Therefore we conclude that the ~ 5.2 day period is probably real.

Due to the uncertainties in the individual period determinations being comparatively large (see Table 1) we could not obtain a more precise value by interpolation between separate data sets (as was previously used with SSI data to refine the period of A1907+09 (ref. 13)). We therefore take the period, based on the weighted mean of the initial determinations to be 5.2 ± 0.3 days. This value is consistent with the known short periods of many luminous X-ray sources and may be interpreted as being due to orbital motion in a close binary system. However, this interpretation meets a serious difficulty concerning the mechanism of mass-transfer.

There is increasing observational evidence that the Sco-like group of sources may simply represent a sub-set of the class of X-ray source, the 'galactic bulge' sources (although this terminology is misleading as many members of the class lie well outside this region—for a review of the galactic bulge sources, and references, see ref. 14). The standard model for this class of X-ray source, which also includes the more exotic objects such as X-ray emitting globular clusters, X-ray bursters and soft X-ray transients, involves the accretion of matter onto a compact degenerate X-ray star (in all probability a neutron star of mass $M_x \sim 1.5 M_\odot$) through the critical Roche lobe overflow of a late-type (later than GV) optical companion of relatively low mass ($M_{\text{opt}} \leq 1 M_\odot$). For a low mass system of this type Roche lobe overflow can only occur for close binaries which have an orbital period $P \leq 0.5$ days. This limit is set by the relationships between the orbital separation a , masses M_x and M_{opt} , and period P :

$$\frac{a}{R_\odot} = 4.21 \left(\frac{P}{1 \text{ day}} \right)^{2/3} \left(\frac{M_x + M_{\text{opt}}}{M_\odot} \right) \quad (1)$$

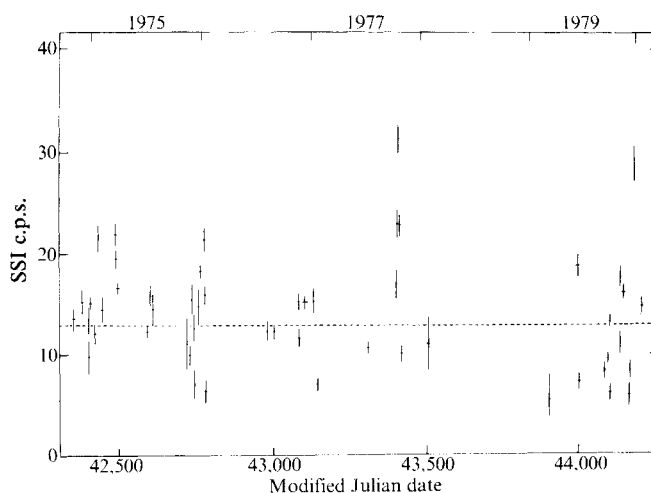
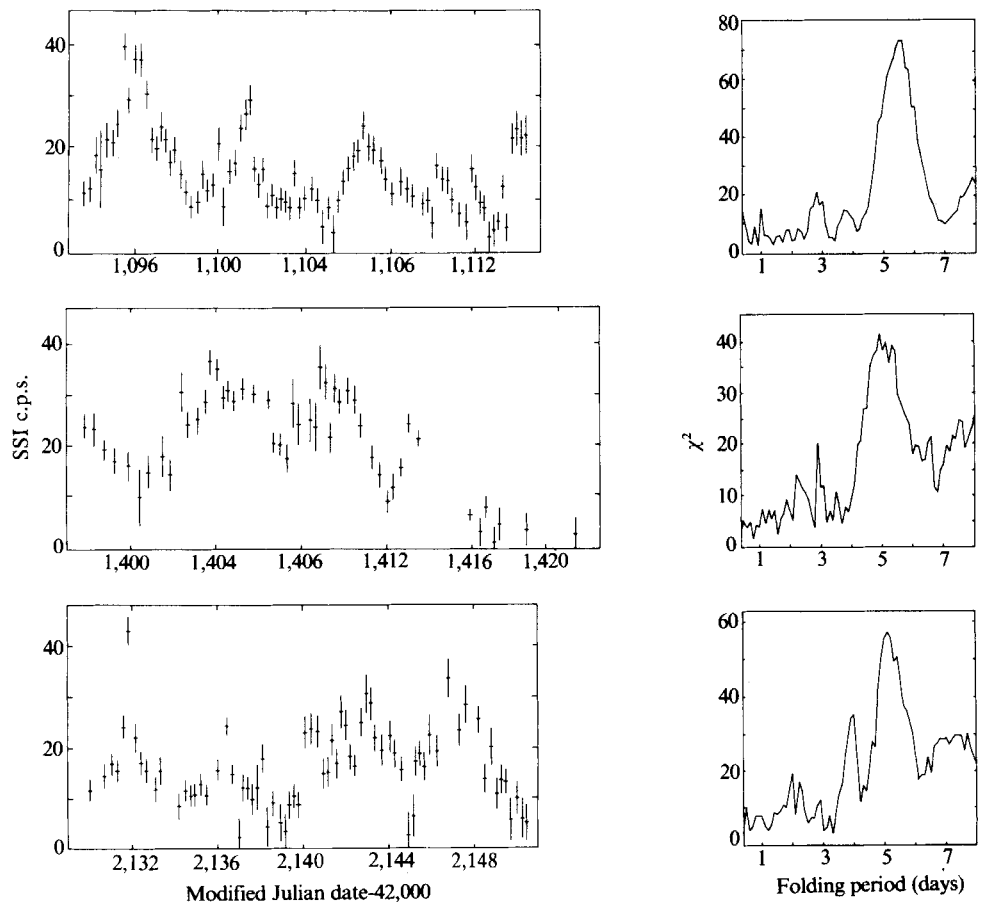


Fig. 1 SSI X-ray observations of 2S0614+091. Each data point represents a summation of data over typically a few days. Errors are $\pm 1\sigma$ and include an estimate of the error due to uncertainties in the spacecraft attitude. The mean of the SSI observations is also shown.

Fig. 2 X-ray observations of 2S0614+091 for data sets 3, 4 and 6, which clearly indicate the ~ 5.2 -day periodicity. Each point represents an integration of data over ~ 5 h. For each data set the corresponding χ^2 versus period distribution (for 9 d.f.) is shown.



and the approximate Roche lobe radius R_L , given by¹⁵:

$$R_L \sim \left[0.38 - 0.2 \log_{10} \left(\frac{M_x}{M_{\text{opt}}} \right) \right] a \quad (2)$$

with the requirement that $R_L \sim R_{\text{opt}}$. Optical observations of the counterpart of 2S0614+091 (refs 5, 8) indicate that it is most probably a late-type dwarf with $M_{\text{opt}} \leq 1 M_{\odot}$ and $R_{\text{opt}} \leq 1 R_{\odot}$ because the spectrum is devoid of ordinary stellar features (that is the optical light is probably dominated by radiation from an accretion disk). For a period of 5.2 days, equations (1) and (2) give the ratio $R_{\text{opt}}/R_L \sim 0.1$, thus ruling out the possibility of Roche lobe overflow.

If the 5.2 day modulation in the X-ray emission from 2S0614+091 does reflect the binary period we must therefore look for an alternative mass-loss mechanism. Substantial mass loss from a system of such low optical luminosity through a strong stellar wind can probably be ruled out (but note strong winds in T Tauri stars¹⁶). The possibility of episodic Roche lobe overflow in an eccentric binary system also seems unlikely, both because of the short circularization time scale for a close binary and because the observed X-ray light curve does not resemble that which would be expected for an eccentric system. Although these arguments make it difficult to see how the 5.2 day period can represent the binary period of this system, note that similar problems have been encountered with the soft X-ray transients A0620-00 and Aql X-1, both of which have established low mass companions^{16,17}. Spectrophotometric measurements and observed high ratios of X-ray to optical luminosities have been interpreted as implying relatively large orbital separations in these systems^{16,18}, again posing problems for the mass transfer mechanism. Indeed, the suggested orbital periods for A0620-00 and Aql X-1 (7.8 and 1.3 days respectively^{19,20}) confirm these relatively large separations and again give critical Roche surfaces too large to be filled by the unevolved late-type

stars. The possibility exists that the X-ray sources in these systems (and in 2S0614+091) are powered by some unknown mode of mass transfer.

An alternative possibility, however, is that the 5.2 day periodicity reported here may not represent the orbital motion of the 2S0614+091 system but rather the precession of an accretion disk around the X-ray star. Such precessing disk models have been used to explain observed periodicities in Her X-1 (refs 21, 22) and suggested for LMC X-4 (ref. 23). Note that for both of these X-ray sources the ratio of the long-term periodicity to the known orbital period is ~ 20 . If a similar situation applies to 2S0614+091 then the orbital period would be ~ 0.25 days for which Roche lobe overflow could then be an entirely plausible mechanism for mass transfer.

Although radial velocity measurements have shown the optical counterpart of Sco X-1 to have a binary period of 0.79 days^{24,25} our result represents the first detection of an X-ray period for a source of the above group of Sco-like variables and therefore provides direct evidence for their binary nature. However, X-ray periods have been proposed for three X-ray sources which are similar in some respects to 2S0614+091. In addition to Aql X-1 (ref. 20) and A0620-00 (ref. 19) the soft X-ray transient Cen X-4 has recently shown evidence for an ~ 8.2 -h periodicity during its decline from maximum outburst²⁶. Note that during their decay both Aql X-1 and Cen X-4 were observed to emit type I bursts^{26,27}. If 2S0614+091 is also a source of type I bursts (as has been suggested from two independent observations of X-ray bursts from its general location^{6,28}) then our result supports the belief that X-ray bursters do occur in close binary systems, the observational evidence for which has so far been elusive.

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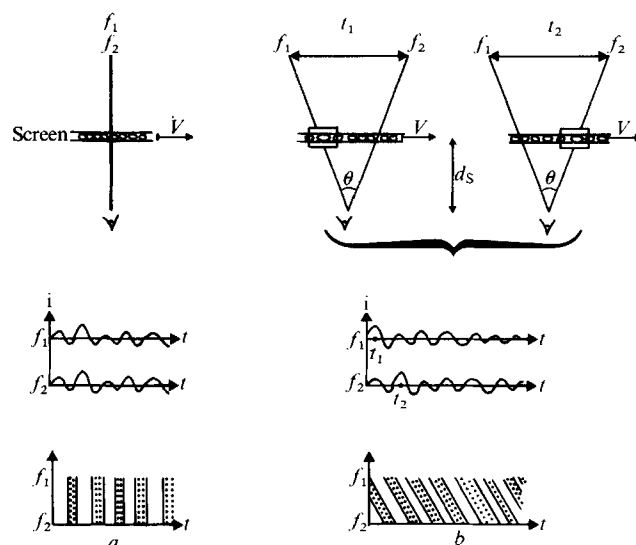


Fig. 1 Principle of the method. *a*, A single source emits at two frequencies f_1 and f_2 , close enough for IPS to be well correlated. Then IPS modulations are simultaneous at the two frequencies. In the case of continuous emission by the source between f_1 and f_2 , features with an infinite frequency drift appear in the dynamic spectra. *b*, Two spatially distinct sources emit radiation at f_1 and f_2 . At instant t_1 , a given part of the scintillating screen (box) produces IPS on radiation at f_1 . Convected by the solar wind, the same part of the screen will be seen later (at instant t_2) in front of the source at f_2 if f_1 and f_2 are again close enough for IPS to be well correlated. Then IPS modulations present a delay:

$$t_2 - t_1 = \frac{\theta d_s}{V_{\parallel}}$$

equal to the time taken by the screen to move between the directions of the two sources. In the case of continuous emission between f_1 and f_2 , drifting features appear in the spectra.

Structure of the source of jovian decametric emission and interplanetary scintillation

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Since its discovery in 1955, Jupiter decametric emission has been extensively studied with ground-based instruments¹, and more recently by planetary radioastronomy experiment on board the Voyager spacecraft^{2,3}. However, we are still far from understanding the origin of the emission, mainly because we have no direct information about the position and structure of the source, due to the low spatial resolution of instruments at those wavelengths. Previous theories have assumed that the source emission takes place along magnetic field lines, close to the local gyrofrequency. We propose here a method to test this hypothesis by studying interplanetary scintillations which modulate the emission when received on Earth, and quantify the predicted effects. Preliminary results indicate that the emission is probably spatially distributed and could occur along field lines, and that Io controlled A and B sources are on opposite sides of Jupiter.

Inhomogeneities of electronic density, convected by the solar wind in the interplanetary medium, produce intensity fluctuations of radio sources called 'interplanetary scintillations' (IPS). They have long been used to study the spatial structure of radio sources in the range of a few tenths to a few arc seconds. On jovian decametric emission they give broadband intensity modulations with a characteristic time of about 1 s (refs 4–6). The principle of our method is illustrated in Fig. 1. If the emission has a continuous frequency spectrum and the source is spatially distributed IPS will appear on the dynamic spectra as a modulation drifting in frequency, with:

$$\frac{df}{dt} = \frac{df}{d\theta} \frac{V_{\parallel}}{d_s} \quad (1)$$

where $V_{\parallel}$ is the solar wind velocity component parallel to the line joining the apparent position of the sources (projection on the sky of the source positions), and the other parameters are defined on Fig. 1.

Our method studies the frequency drift of IPS or the time delay between IPS recorded at two different frequencies. From equation (1) we could obtain the apparent distance between the sources, projected on the solar wind direction, provided the other parameters $V_{\parallel}$ and d_s are known. The limits of the method will be due to the precision in our knowledge of $V_{\parallel}$ and d_s .

$V_{\parallel}$ can be calculated from the solar wind velocity and the geometry of the observation (solar elongation ϵ , screen position, solar wind direction, apparent direction of the sources). The change in velocity with solar wind activity must be taken into account. But for the preliminary study we have used a velocity of 400 km s⁻¹ which is an average during periods of low solar activity. The distance to the scattering screen is more difficult to determine. At low frequencies, except for low elongations, we cannot assume that the screen is thin and near the point of closest approach. We have used computations for IPS at 34 MHz (ref. 7) and, for a first calculation, assume that the screen is concentrated at the distance which gives the maximum contribution to IPS in the model.

With $V_{\parallel}$ and d_s approximated as described above, we can give an order of magnitude of drift rates, taking the example of Io-controlled A and B sources¹, which are assumed to occur close to the local gyrofrequency along Io field line which is east of Jupiter for Io-B emission (central meridian longitude (CML) $\sim 140^\circ$, $\phi_{10} \sim 90^\circ$) and west for Io-A emission (CML $\sim 230^\circ$, $\phi_{10} \sim 240^\circ$). The magnetic field is fairly well known from Pioneer 11 measurements. The results from O₄ (ref. 8) and P11 (ref. 9) models give similar results. The absolute values of drift rate calculated from equation (1) are listed in Table 1, for both

Table 1 Absolute values of drift rate

ϵ°	Predicted absolute drift rate at 23 MHz (MHz s ⁻¹)	
	Io-A	Io-B
30	4.0	3.2
60	6.4	5.0
90	7.0	5.5
120	9.4	7.4

sources and different solar elongations. We conclude that the effect leads to detectable delays. Moreover, with existing theories of jovian decametric emission, we can predict some simple qualitative effects which must be observed on IPS.

(1) For given source, the sense of drift must reverse with the solar wind direction (with $V_{\parallel}$).

(2) Sources on the east and west sides of Jupiter will have opposite drift senses (Fig. 2a, c). A source near the CML will not show any drift (Fig. 2b).

(3) If the source is on a field line rotating with the planet, the frequency drift will change during the few hours that the source is observed.

(4) If the angular distance between the sources is frequency dependent (as in the case of emission along a field line, close to the local gyrofrequency), so is the frequency drift.

Preliminary tests of the method have been performed. Wide band spectral observations of Jupiter decametric emission are continuously made in Nançay (France), with a very sensitive array¹⁰. From dynamic spectra which often show the presence of drifting IPS (Fig. 3) we can conclude that:

The existence of measurable drift rates means that the source is distributed in space.

In Fig. 3a, the absolute drift rate increases with decreasing frequency, in accordance with a source along a field line.

For given solar wind direction, the drift sense is different for Io-A (Fig. 3a, c) and Io-B sources (Fig. 3b, d) and, due to the geometry of these four observations, corresponds to a Io-A source on the west side of Jupiter and Io-B source on the east side (the same side as Io in both cases).

For a given jovian source, the sign of the drift rate reverses with the solar wind direction (Fig. 3a compared with c, Fig. 3b compared with d), as predicted above.

The magnitudes of drift rates in Fig. 3a, b for example are about -1 MHz s^{-1} and 4.5 MHz s^{-1} respectively, to be compared with about -7.5 MHz s^{-1} and 7 MHz s^{-1} predicted from (1) if emission occurs along Io field line close to the local gyrofrequency, with the above defined approximations for d_s and $V_{\parallel}$. This discrepancy is not very surprising, as we used rough

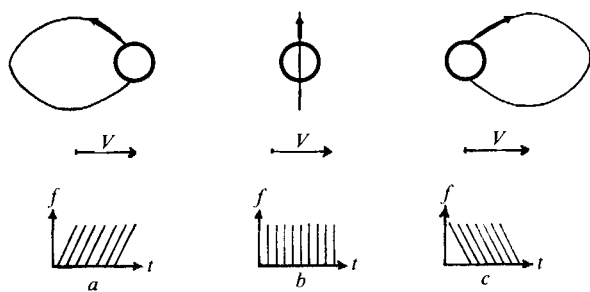


Fig. 2 IPS observed in three configurations of the source, with the same solar wind direction. The source is supposed to lie along a field line (seen in projection on the sky): a, on the east side of Jupiter; b, at the central meridian longitude; c, on the west side of Jupiter. The arrows indicate the direction of decreasing frequencies along the field line.

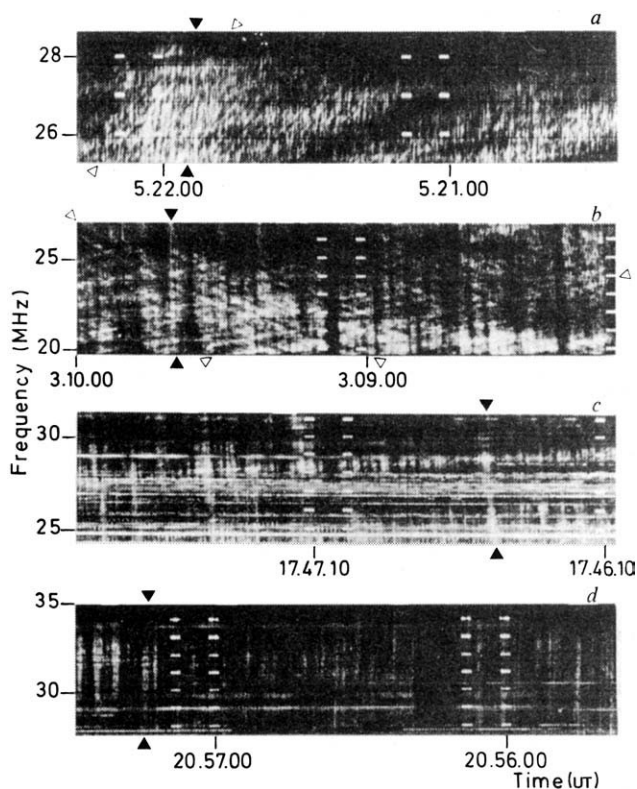


Fig. 3 Dynamic spectra of jovian emission observed in Nançay. IPS direction is indicated by black arrows. The direction of the jovian 'modulations lanes', which cross the spectra with a far lower drift rate, is indicated by white arrows. a, Io-A source (CML $\sim 225^\circ$, $\phi_{10} \sim 243^\circ$) observed on 17 November 1980 at solar elongation $\epsilon \sim 51^\circ \text{E}$. IPS drift negatively. b, Io-B source (CML $\sim 157^\circ$, $\phi_{10} \sim 95^\circ$) observed on 4 February 1981 at $\epsilon \sim 125^\circ \text{E}$. IPS drift positively. c, Io-A source (CML $\sim 228^\circ$, $\phi_{10} \sim 228^\circ$) observed 22 June 1981 at $\epsilon \sim 91^\circ \text{W}$. IPS drift positively. d, Io-B source (CML $\sim 133^\circ$, $\phi_{10} \sim 99^\circ$) observed on 23 June 1981 at $\epsilon \sim 90^\circ \text{W}$. IPS drift negatively.

approximations of $V_{\parallel}$ and d_s , but further measurements will be necessary to decide whether it is significant.

These preliminary observations show that IPS drifts can be explained by the spatial structure of the jovian source. They also give a very important check of the existing theory: the Io-controlled sources are spatially distributed with frequency (as predicted for an emission along a magnetic field line) and A and B sources are on opposite sides of Jupiter (as predicted if they occur along Io field line). However, the determination of drift rates from analogue records such as those of Fig. 3 is not accurate. A numerical cross-correlation method will be used in the future for an exhaustive quantitative study of the sources of jovian emission. Comparison of Io-controlled and non-controlled emissions will be of special interest, as no satisfactory explanation has been proposed for the non-Io controlled sources.

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The generation mechanism for magnetosheath lion roars

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'Lion roars' are intense electromagnetic whistler-mode waves which occur throughout the magnetosheath¹. The emissions have a mean frequency near a few tenths of the electron gyrofrequency, a typical amplitude of 0.1 nT and occur in bursts with an average duration of a few seconds². When played through a loudspeaker the emissions sound like a lion roaring, hence the name. We report here that analysis of simultaneous wave and plasma data obtained on the ISEE-1 satellite indicates that the waves can originate by cyclotron resonant instability with anisotropic magnetosheath electrons whenever the magnetic energy per particle, $B^2/8\pi N$, falls to values comparable with the electron thermal energy. The variation in this critical energy for instability seems to be associated with the hydromagnetic mirror instability which in turn is excited by the observed pressure anisotropy in the magnetosheath ion population.

Several mechanisms have previously been suggested² for the origin of these intense emissions, each involving Doppler-shifted resonance at integral ($n = 0, \pm 1, \dots$) multiples of the resonant particle gyrofrequency:

$$\omega - K_{\parallel} V_{\parallel} = n\Omega_{\pm} \quad (1)$$

In typical magnetosheath conditions (density, N , $\sim 20 \text{ cm}^{-3}$, magnetic field, B , $\sim 20 \text{ nT}$) the characteristic scaling energy for wave-particle resonance $E_M = B^2/8\pi N \approx 50 \text{ eV}$. Over the observed wave frequency band ($\omega/\Omega_{-} \approx 0.1-0.5$) the dominant cyclotron ($n=1$) resonant electrons have energies $E_{\text{cyc},-} \approx E_M(\Omega_{-}/\omega)(1-\omega/\Omega_{-})^3$ (refs 3, 4) of 10–300 eV while Landau ($n=0$) resonant electron energies $E_{L,-} \approx E_M(\omega/\Omega_{-})(1-\omega/\Omega_{-})$ are $\sim 10 \text{ eV}$. As both are comparable with the electron thermal energy ($E_{\text{th}} \sim 30 \text{ eV}$), a significant fraction of the magnetosheath electrons can resonate with the waves, leading to rapid wave growth or damping. Alternatively, cyclotron or Landau resonance with magnetosheath protons requires energies $E_{\text{res},+} \approx (M_{+}/M_{-})E_{L,-} \approx 10-20 \text{ keV}$.

Because the observed waves typically propagate within 15° of the ambient magnetic field direction², the parallel component of the fluctuating wave electric field is small and Landau resonance is therefore ineffective as a generation mechanism^{3,4}. However, Landau damping, particularly by thermal electrons, probably explains why the waves are rarely observed with highly oblique wave normal angles. One natural candidate for the origin of parallel propagating waves is cyclotron resonant instability; the free energy for wave growth can be provided either by the pitch-angle anisotropy of low energy magnetosheath ($>10 \text{ eV}$) electrons or by field-aligned beams of energetic (10–20 keV) streaming protons². Both are viable sources because the dayside magnetosheath is observed to have a persistent pressure anisotropy⁵ of the correct sign ($p_{\perp} > p_{\parallel}$) for whistler-mode wave growth, and field-aligned ion beams are consistently observed on the ISEE satellites just outside the magnetopause streaming away from the magnetosphere⁶. Particle and field data obtained on the ISEE satellites are used here to determine whether either of these potential generation mechanisms can account for lion roars.

Smith and Tsurutani² earlier established a strong correlation between the occurrence of lion roars and decreases in the ambient magnetosheath magnetic field. Figure 1 illustrates a particularly dramatic example of such a relationship from data

taken over a 3-min interval on 8 November 1977, when the ISEE satellite was located in the magnetosheath at $11.8 R_E$ near 11.20 LT. The lower panels exhibit six channels of the fluctuating plasma wave magnetic field. Each channel has a bandwidth $\approx \pm 15\%$ of the centre frequency⁷ and the intensity is plotted on a logarithmic scale in units of $\text{nT}^2 \text{ Hz}^{-1}$. The total plasma density and the ambient magnetic field are plotted in the upper panels and for comparison the magnetic field strength is superimposed on the 100-Hz wave channel. Two intense lion roars are observed centred near 02.30:42 and 02.31:25 UT and a smaller burst occurs at 02.32:35 UT. The peak wave intensity typically occurs below 100 Hz and the emission bandwidths are relatively broad. The emission at 02.30:42 UT is primarily confined to frequencies above 30 Hz and detectable wave intensities are found at frequencies up to 311 Hz. The burst at 02.31:25 UT extends to frequencies as low as 17 Hz and is mainly confined to

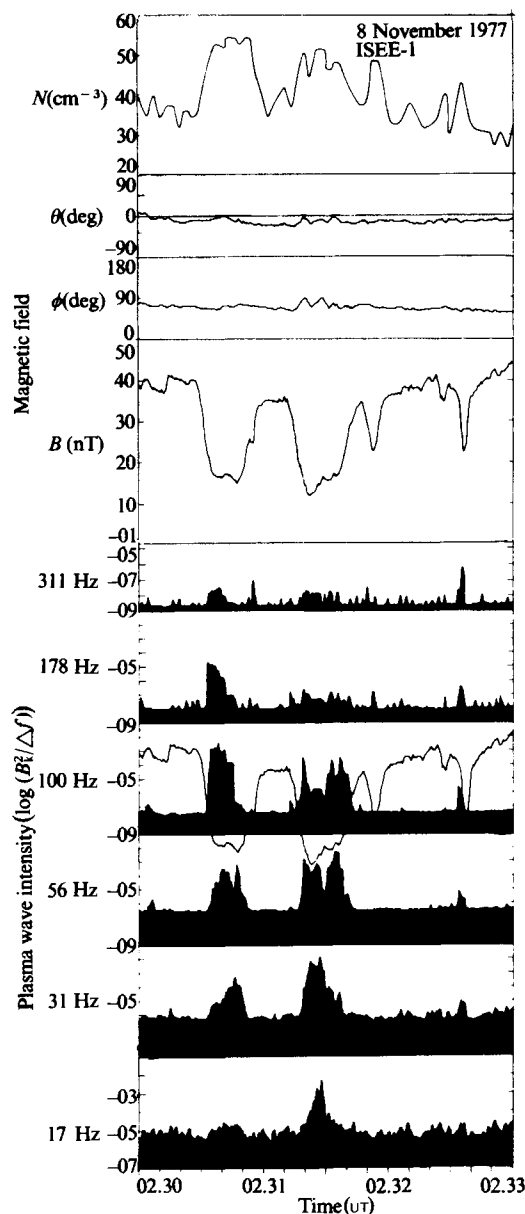


Fig. 1 The upper panels exhibit the plasma density and magnetic field variability observed in the magnetosheath on ISEE-1. Simultaneous spectrum channel data from the plasma wave experiment are plotted in the lower panels. The spectral intensity in each channel is plotted on a logarithmic scale in units of $\text{nT}^2 \text{ Hz}^{-1}$. Intense lion roar emission is observed during decreases in the ambient magnetic field, here superimposed on the 100-Hz channel.

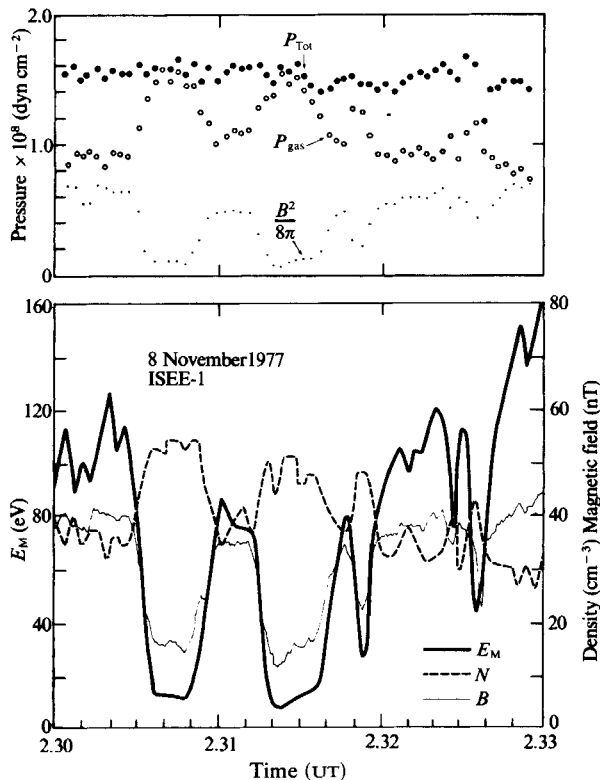


Fig. 2 The lower panel exhibits the pronounced anticorrelation between magnetic field strength and plasma density variation during the interval of lion roar emission. Total plasma pressure (top panel) remains essentially constant over this interval, suggestive of a slow mode hydromagnetic wave. The scaling energy for electromagnetic wave-particle interaction E_M varies by over an order of magnitude and lion roar emission occurs when $E_M \approx E_{th} \approx 30$ eV.

frequencies below 100 Hz. The short burst at 02.31:25 UT is more intense at higher frequencies, with significant power up to 311 Hz.

Note that the two major events have a duration (≈ 20 s) much longer than the 'typical' lion roars described earlier by Smith and Tsurutani². These extraordinarily long-duration events were specifically chosen for our study so that complementary plasma measurements could be used in the analysis. The plasma instrument takes ~ 3 s to complete an energy and angular scan⁸. This time resolution is adequate for studying these long-duration events, but would not be possible for the more typical short duration ($\approx$ few seconds) lion roars.

Tsurutani *et al.*⁹ have undertaken a detailed analysis of the plasma distribution function during this interval of lion roar emission and these experimental results will be used here to identify the mechanism for wave growth. The magnetosheath electrons have a thermal energy E_{th} of ≈ 25 –30 eV, with $T_{\perp} \geq T_{\parallel}$ throughout the interval of analysis. Due to the method of sampling, the electron thermal anisotropy $A_{\perp} = T_{\perp}/T_{\parallel} - 1$ could not be precisely determined, but a minimum value $A_{\perp} \geq 0.2$ was established during the period of intense lion roar emission⁹. It is also important to realize that the electron anisotropy did not vary adiabatically, in contrast to the anticipated behaviour during a hydromagnetic wave oscillation. However, there is no reason to expect an adiabatic evolution in the electron anisotropy because Tsurutani *et al.*⁹ have determined that the observed plasma and field variations represent distinct spatial structures convecting past the ISEE-1 and -2 spacecraft rather than large-scale temporal oscillations. Magnetosheath protons had considerably larger thermal energy ($T_{\perp} \approx 200$ eV) with a persistent pressure anisotropy ($p_{\perp} > p_{\parallel}$) similar to that of the

electrons. A bulk plasma flow velocity ≈ 65 km s $^{-1}$ was determined from the plasma data but no evidence was found for energetic (≥ 10 keV) field-aligned beams of ions moving relative to the main bulk of the plasma. This latter result is true for all lion roars studied so far.

Each of the exhibited lion roar bursts are correlated with decreases in the magnetic field intensity, in agreement with previous work. The main difference between these cases and the events previously discussed in the literature is the magnetosheath field variability which is much larger here. The plasma density exhibits a strong anticorrelation with the magnetic field variability as shown in the lower panel of Fig. 2, and despite the large variability in B and N the total plasma pressure ($B^2/8\pi + NkT$) remains essentially constant over this interval (upper panel). This is further proof that we are dealing primarily with a convecting spatial structure rather than with a wave oscillation. The combination of increased plasma density and lower magnetic field also produces a dramatic decrease in the scaling energy $B^2/8\pi N$ for resonant wave-particle interactions. This scaling energy drops from values ≈ 100 eV outside the events to as low as 10 eV during the intervals when the lion roars are observed. Because the electron thermal energy is comparable to 30 eV during this period, one can anticipate an exponential increase in the number of electrons capable of resonating with the waves and a corresponding enhancement in the wave growth rate as evaluated below.

Because of the absence of streaming ions, emphasis here is placed on cyclotron resonant instability driven by the persistent pitch-angle anisotropy in the electron distribution. Although a formal simulation of the wave growth should use the exact warm whistler-mode dispersion relationship¹⁰ and also include the effects of nonlinear feedback of the waves on the resonant particle population¹¹ for the present purpose of simply identifying the dominant mechanism for wave growth we adopt linear cool plasma theory which provides an approximate but analytic solution for the temporal growth rate¹²

$$\gamma_{cyc} \equiv \pi \Omega_{\perp} (1 - \omega/\Omega_{\perp})^2 \eta_{\perp}(V_R) (A_{\perp}(V_R) - A_c)$$

where $\eta_{\perp}(V_R)$ is the fractional number of resonant electrons, and $A_c = (\Omega_{\perp}/\omega - 1)^{-1}$ is the critical anisotropy required for instability. If the electrons are modelled by a bimaxwellian distribution

$$f(\vec{V}) \sim \frac{1}{\pi^{3/2}} \frac{1}{a_{\perp}^2 a_{\parallel}} \exp \left\{ - \left[\frac{V_{\perp}^2}{a_{\perp}^2} + \frac{V_{\parallel}^2}{a_{\parallel}^2} \right] \right\}$$

where $a_{\parallel, \perp}$ are the thermal velocities parallel and perpendicular to the ambient field, the pitch-angle anisotropy $A_{\perp}(V_R) = (T_{\perp} - T_{\parallel})/T_{\parallel}$ is simply a function of the electron temperatures $T_{\parallel, \perp}$ and the wave convective growth rate is

$$K_i = \gamma/V_g = \frac{\pi^{1/2}}{2} \frac{\omega_p}{c} \left(\frac{A_{\perp} - A_c}{A_c^{1/2}} \right) \left\{ \frac{V_R}{a_{\parallel}} \exp \left(- \frac{V_R^2}{a_{\parallel}^2} \right) \right\} \quad (2)$$

where ω_p is the plasma frequency and v_R is the parallel resonant electron velocity (equation (1)).

The exponential factor in equation (2) leads to greatly enhanced convective growth in regions where the resonant electron energy becomes comparable with or smaller than the electron thermal energy. To illustrate how dramatic this change in wave growth can be, the convective amplification in wave power ($2K_i$) has been evaluated for three selected intervals corresponding to the minimum and maximum of the ambient magnetic field oscillations. The net amplification (in dB R_E^{-1}) is plotted as a function of wave frequency and electron anisotropy in Fig. 3. The two examples with large convective wave growth (at 02.30:42 and 02.31:25 UT) lie within regions of low magnetosheath field ($B \leq 20$ nT), where $E_M = B^2/8\pi N$ is less than the electron thermal energy (Fig. 2). As the unstable waves resonate with the main bulk of the plasma the amplification is

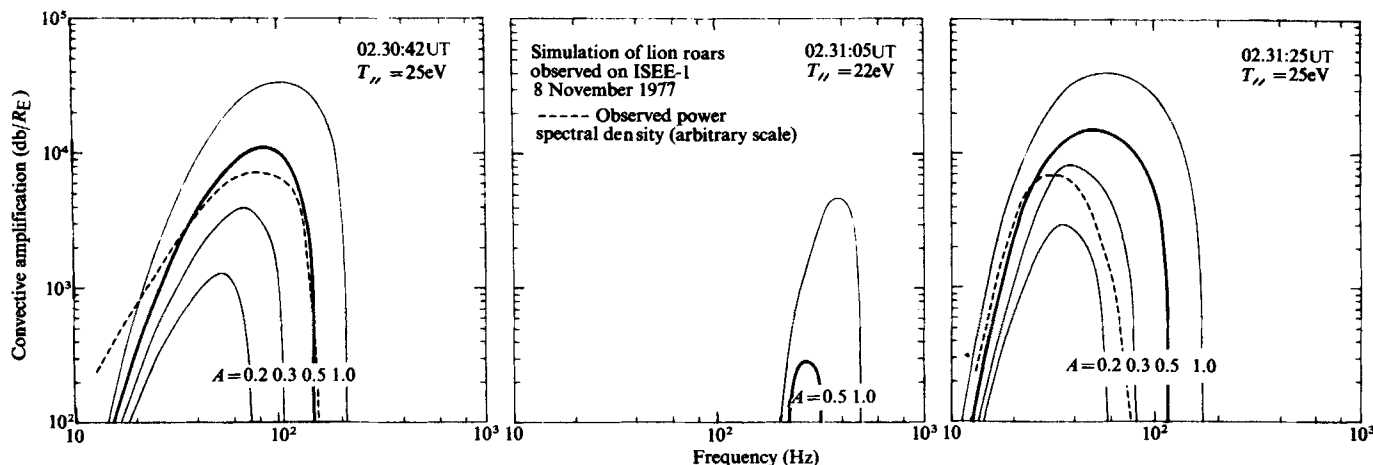


Fig. 3 The convective gain in wave power plotted against wave frequency for three selected time intervals. The two examples with large wave gain occur during the two major lion roars in Fig. 1. The observed relative power spectral intensity is indicated by the dashed lines and may be used to infer indirect information on the electron anisotropy.

very rapid. In fact, for the examples shown, the linear growth rate (2) cannot strictly be used because the peak temporal growth rate becomes comparable with the wave frequency. The waves should therefore be rapidly amplified until nonlinear processes limit the growth to produce the observed spectrum. In contrast, at 02.31:05 UT, the anticipated wave growth is substantially smaller primarily because the waves now resonate with superthermal electrons ($E_{\text{res}} \gg E_{\text{th}}$) which are less abundant. The net amplification is apparently insufficient to produce observable waves.

A precise determination of the electron pitch-angle anisotropy is important because it not only provides the source of free energy for instability but also establishes an absolute upper limit on the frequency of unstable waves, $(\omega/\Omega_-)_{\text{max}} = A_-/(1+A_-)$. This upper limit is not modified by warm plasma corrections to the wave distribution function¹⁰. Also, because the resonant electron velocity increases as ω/Ω_- decreases, the convective growth rate (2) will decrease exponentially at low frequency. This effectively limits the most unstable waves to a frequency band just below the upper cutoff. The observed lion roar frequency spectrum might therefore provide indirect information on the electron anisotropy. To test this hypothesis we have superimposed the average wave power spectral densities observed during the two intense lion roars on the theoretical linear convective growth rates. At 02.30:42 UT the agreement between observation and theory is best for $A_- \approx 0.5$ whereas at 02.31:25 UT a value of $0.2 \leq A_- \leq 0.3$ is suggested. Both are consistent with the lower limit on the electron anisotropy ($A_- \geq 0.2$) set by the plasma observations⁹. At 02.31:05 UT the absence of detectable waves is consistent with an electron anisotropy $A_- \leq 0.5$.

The concept that variations in the scaling energy for electromagnetic wave particle resonance, $B^2/8\pi N$, might influence plasma instability was initially discussed as a mechanism for the enhanced precipitation loss of energetic ring current particles due to the onset of either electromagnetic ion-cyclotron¹³ or whistler-mode¹⁴ instability. It has even been invoked as a means of triggering magnetospheric substorms by artificially injecting small amounts of cold plasma¹⁵. However, we believe the results presented here to be the first explicit demonstration (using simultaneous wave and particle data) of how wave instability can be modified by changes in $B^2/8\pi N$. In the examples analysed, the wave onset is particularly dramatic because the resonant electron energy in the magnetosheath oscillates around the electron thermal energy, thereby causing an exponential variation in wave growth rate. The confinement of emissions to regions where $E_M \leq E_{\text{th}}$ also indicates that lion roars are strongly controlled by local plasma properties. Although the waves are undoubtedly convective, Landau damping should become

important for obliquely propagating waves. Convective damping rates for $\theta \geq 20^\circ$ can exceed $100 \text{ dB } R_E^{-1}$, thus confining the observed waves to the maximally unstable regions.

Because the emissions analysed here are somewhat atypical, one should be cautious in generalizing these results to imply that lion roars are always excited by cyclotron instability with anisotropic magnetosheath electrons. Nevertheless, this source of free energy is permanently maintained by the convective transport of magnetosheath plasma from the shock to the magnetopause¹⁶. When $\beta = 8\pi NkT/B^2$ is large, the observed positive ion anisotropy can excite a mirror instability¹⁷ which requires $\beta_\perp/\beta_\parallel > 1 + (1/\beta_\perp)$. However, as the wave frequency is purely imaginary, the mirror instability is strictly non-oscillatory¹⁸. It will simply tend to enhance the plasma density irregularities in the medium while conserving total pressure. This is consistent with the large density and field fluctuations shown in Fig. 2. As the region of the magnetosheath just ahead of the magnetopause generally satisfies the condition for mirror instability of the slow mode compressional wave¹⁹, one can anticipate the development of irregularities in $B^2/8\pi N$ which in turn can trigger high-frequency electromagnetic lion roar emissions once the electron thermal energy becomes comparable to E_M . Lion roars can thus be thought of as a natural consequence of the quasi-adiabatic transport properties of the magnetosheath.

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On the validation of one-dimensional CFC-ozone depletion models

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There has been considerable progress in understanding the possible impact of the continued release of chlorofluorocarbons (CFCs) on the stratospheric ozone layer. Current estimates of the steady-state ozone depletion due to the release of CFCs 11 and 12 at the 1976 production rates are ~10%; the recent decrease is due largely to revisions in some OH and HO₂ reaction rate coefficients¹. There have been changes in the calculated vertical profiles in the stratosphere of many of the trace species considered as essential components of the models associated with this downward revision in the predicted ozone depletion. Comparison of such vertical profiles calculated for the present, relatively unperturbed atmosphere with available observations is the only experimental means of verifying the models. We show here that the accuracy and coverage of the observations are insufficient to distinguish between models giving predicted steady-state ozone depletions differing by at least a factor of 2. The same calculations show that the largest differences in vertical profiles occur for the temporary reservoir species HOCl, HO₂NO₂, H₂O₂ and ClONO₂ for which virtually no data are available. Measurements of these reservoir species would significantly improve our ability to validate current models.

The model experiments described here were performed with the AERE Harwell one-dimensional (1-D) model, the details of which are given elsewhere^{2,3}. The chemical kinetic and photochemistry input data for the 123 processes are taken from the NASA-JPL evaluations^{4,5} and have been updated with the following rate coefficients to obtain 1981 input data. (OH + HNO₃ 4.6×10^{-12} cm³ molecule⁻¹ s⁻¹ (ref. 6); OH + HO₂ 6.0×10^{-11} cm³ molecule⁻¹ s⁻¹ (refs 7, 8); and ClO + NO₂ one-quarter of measured low pressure third-order rate coefficient⁵). The differential equations describing the 49 model species were expressed in finite-difference form and integrated on a 2-km grid covering 11–51 km using a variable order Gear's method program, FACSIMILE⁹. A constant-Sun approximation was used to average over diurnal and seasonal variations in photolysis rates. The illumination, pressure and temperature conditions of 30 °N or S were used although a global description of net vertical exchange through a common *K_z* profile was specified for all species.

Perturbation experiments were performed setting up a natural atmosphere simulation using methyl chloride and carbon tetrachloride as stratospheric chlorine sources and integrating the model equations for 10¹⁰ model seconds. This natural atmosphere simulation took about 4 min CPU with the Harwell IBM 3033 computer, and contained ~1.2 p.p.b. (1 p.p.b. = 1 part per 10⁹ by volume) odd chlorine in the upper stratosphere in steady state. This natural atmosphere was then perturbed by injecting CFCs 11 and 12 at the 1976 production rates until a new steady state was established. The ozone depletion was then determined from the difference in the total ozone columns between the perturbed and natural atmosphere simulations. The steady-state ozone depletion estimates obtained with the AERE Harwell 1-D model for CFC 11 and 12 release are heavily dependent on the formulation of stratospheric chemistry. Over recent years, the many re-evaluations of critical chemical rate coefficients have caused marked changes in the steady-state ozone depletion estimates as shown in Fig. 1. Also shown are some of the depletion estimates taken from earlier reports on the ozone layer. It is difficult to avoid the

conclusion that uncertainties in chemistry have dominated recent changes in ozone estimates. The variations in the ozone depletion estimates indicated in Fig. 1 do not reflect adequately the likely uncertainty range in the central value. This could only be obtained from a detailed treatment of the uncertainties in the solar flux, reaction rate coefficients, boundary conditions, transport coefficients and source distributions. Such a treatment is not yet available.

At present, the most important check on model performance lies in the comparison of model profiles with the available observations for those trace species which figure prominently in the models. Whilst these comparisons are limited by the sparsity and incompleteness of the available data and by the averaging inherent in the model formulation (latitude, longitude, diurnal and seasonal), such comparisons are important in assessing the performance of models as research tools. To go further and to check the validity of the model perturbation studies requires a more than superficial comparison of model with measurement, a more detailed treatment of model uncertainty and the removal of the subjective elements of model comparison with measurement. Whilst similar views have been expressed previously¹⁰, the recent changes in CFC-ozone depletion estimates warrants this further consideration.

We attempt here to indicate the necessary requirements, placed on atmospheric measurements in terms of accuracy and coverage, if they are to be used to validate models of CFC-ozone depletion. The natural atmosphere simulations from two perturbation experiments using 1979 chemistry⁴ and 1981 chemistry⁵ input data have been compared. These experiments

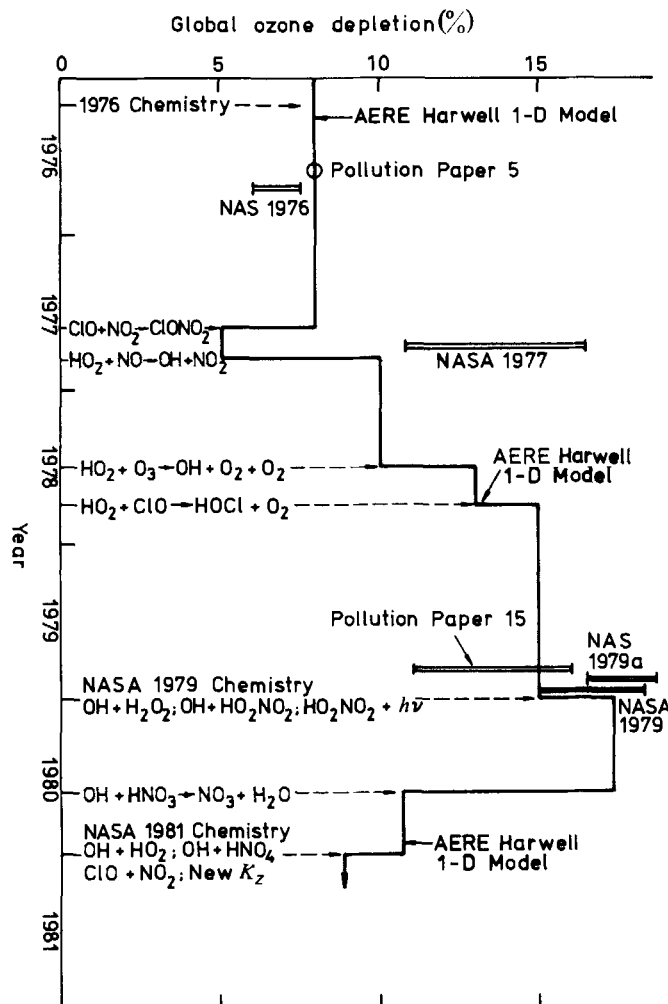


Fig. 1 The steady-state total column ozone depletion given by the AERE Harwell 1-D model for constant release of CFCs 11 and 12 at 1976 rates, showing the effects of major revisions in the chemistry. Some results from major reports¹²⁻¹⁷ on the ozone layer are also included.

Table 1 Comparisons of the altitude profiles of the various species in the natural atmosphere simulations with 1979 and 1981 model input expressed as ratios

Altitude (km)	Stratospheric source species								Long-lived and sink species							
	CH ₄	H ₂	H ₂ O	CO	N ₂ O	CH ₃ Cl	CCl ₄		O ₃	NO	NO ₂	HNO ₃	HCl	COCl ₂		
12	1.0*	1.0*	1.0*	1.0*	1.0*	1.0*	1.0*		0.97	1.0*	1.00*	0.53	0.41	0.84		
20	0.97	1.00	1.02	0.58	0.98	0.85	1.01		1.05	0.39	0.42	1.35	0.76	0.83		
30	0.99	1.00	1.00	0.38	1.07	0.70	1.35		1.08	0.73	0.79	1.24	0.53	1.04		
40	1.04	1.00	0.99	0.98	1.20	0.72	2.01		1.00	0.94	0.96	1.11	0.99	1.48		
50	1.00	0.98	1.00	0.97	1.11	0.57	1.19		0.93	0.96	1.00	1.00	0.98	1.07		
Reactive species																
	O(³ P)	O(¹ D)	O ₂ (¹ Δ _g)	OH	HO ₂	H	CH ₃	CH ₃ O ₂	CH ₃ O	HCO	HCHO	N	NO ₃	Cl	ClO	
12	0.97	0.92	0.97	2.11	2.17	2.10	2.08	2.53	0.27	2.43	2.01	—	1.60	1.47	1.32	
20	1.05	1.00	1.05	3.05	6.29	1.84	2.42	6.12	0.24	2.92	2.08	—	0.82	2.36	5.92	
30	1.08	1.03	1.08	1.47	2.05	1.45	1.21	1.66	0.19	1.38	0.96	0.75	1.07	0.71	0.97	
40	1.00	0.95	1.00	1.11	1.31	1.25	1.09	1.16	0.25	1.14	0.88	0.94	1.01	1.08	1.08	
50	0.93	0.89	0.93	1.02	1.35	1.11	0.95	0.99	0.29	0.99	0.83	0.97	0.93	0.96	0.95	
Temporary reservoir species																
	HO ₂ NO ₂	H ₂ O ₂	N ₂ O ₅	CH ₃ O ₂ NO ₂	CH ₃ O ₂ H	HOCl	ClONO ₂									
12	2.46	4.64	1.60	2.53	1.08	0.96	5.54									
20	3.00	32.88	0.34	2.58	4.91	11.69	9.90									
30	2.35	3.54	0.84	1.31	0.58	0.84	3.03									
40	1.46	1.63	0.97	1.12	0.44	0.79	4.11									
50	1.36	1.79	0.92	0.98	0.68	0.83	3.89									

Ratio expressed as number density in 1979 model divided by corresponding value in 1981 model natural atmosphere simulation.

* Comparison affected by boundary conditions.

gave steady-state ozone depletion estimates which differed by a factor of 2 from 18% to 9%, respectively (see Fig. 1). All other aspects of model formulation remained closely similar although this was not a specific aim. Rather, we have made all changes necessary to maintain the steady improvement in model performance over the period of Fig. 1. This includes a recent change in K_z profile from one based largely on the observed CH₄ profile (Chang 1976)¹¹ to one based on N₂O (ref. 3). However, this change and those due to the re-evaluations in the photochemistry input not marked in Fig. 1 had little significance in comparison with the major effects of the re-evaluations in some of the OH and HO₂ rate coefficients.

Table 1 shows the differences in the model profiles of the number densities of the O_x, HO_x, NO_x and ClO_x species in these two different natural atmosphere simulations. The O₃, O(³P) and O₂(¹Δ_g) profiles (Table 1) all show similar fractional changes of 8% or less in concentration at corresponding altitudes; O(¹D) shows different relative changes of 8% or less except for a slightly larger difference at 50 km. It is unlikely that global mean atmospheric measurements of the O_x family could be made over the next few years with the accuracy required to distinguish between the two models despite the fact that they gave steady-state ozone depletion estimates differing by a factor of 2.

Figure 2 shows that the differences in the calculated profiles of O₃ and CH₄ are negligible in comparison with the error bars on the present available measurements. Table 1 gives further data

for the HO_x family. The calculated differences for H₂O and H₂ are small although for CO the recent changes in chemistry have markedly affected its distribution in the lower stratosphere through changes in the OH distribution. Unfortunately, some of the CO measurements are unreliable in the lower stratosphere. The free radical species exhibit significant changes of a factor of ~3 in the lower stratosphere where there are few measurements. H₂O₂, a temporary reservoir species, shows the largest single difference in this family, again in the lower stratosphere. Significant differences across the whole of the stratosphere were found for both H₂O₂ and CH₃O₂H (methyl hydrogen peroxide).

Table 1 shows that the altitude profile of N₂O, the major NO_x source species, has changed only slightly although there are some changes in the lower stratosphere. With the NO_x family, again, the temporary reservoir species N₂O₅, HO₂NO₂ show the largest sensitivity. The sink species HNO₃ shows relatively little sensitivity to the changes in chemistry although OH + HNO₃ has suffered a major re-evaluation (see Fig. 2). The available measurements have insufficient accuracy for us to choose between either calculated profile.

Table 1 shows that the profiles of the stratospheric source species CH₃Cl and CCl₄ have been slightly changed by the re-evaluations although the other ClO_x species show much more significant changes. The distribution of the free radical species and temporary reservoir species have all been significantly changed, particularly in the lower stratosphere. The largest

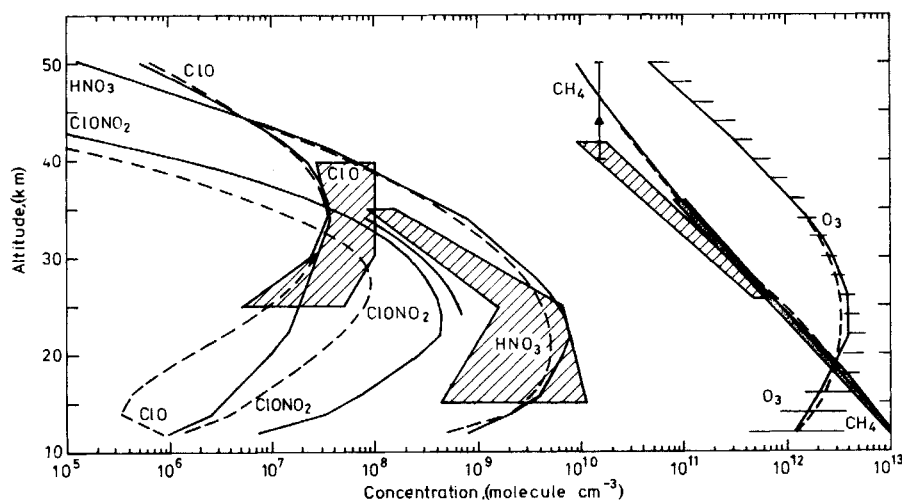


Fig. 2 The calculated altitude profiles of a number of trace constituents in the stratosphere using 1979 (solid line) and 1981 (dashed line) chemistry input data in the AERE Harwell 1-D model. Also shown are the ranges of the available observations¹²⁻¹⁹.

changes are observed with the temporary reservoir species HOCl and ClONO₂. Figure 2 illustrates the observed range in the available measurements of ClO. Unfortunately measurements have not been made in the lower stratosphere where the models indicate most of the recent changes have occurred. Again, measurements are unable to distinguish between either of the calculated profiles. Although large differences are predicted for ClONO₂ (Fig. 2), only a few isolated measurements are available¹².

We conclude that of all the species, the predicted profiles of temporary reservoir species have changed the most as a result of recent changes in chemistry. Although these species should be sensitive indicators of the validity of the formulation of the chemistry of the lower stratosphere in the models, they have not yet been reliably and adequately measured in the atmosphere.

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For many of the other reactive species, the available altitude profiles do not have the accuracy and coverage for us to choose between either of the natural atmosphere simulations which gave ozone depletions which differed by a factor of 2. The differences in the predicted altitude profiles of the stratospheric source gases were found to be small so that although measurements of these species are important in the formulation of the models, they will not have a vital role in model validation. This latter role would apparently be best served by the temporary reservoir species HO₂NO₂, H₂O₂, ClONO₂, N₂O₅ and HOCl when detailed measurement coverage should become available.

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Climatic signal of ice melt features in southern Greenland

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The stratigraphic record of melt features in intermediate depth polar ice cores has provided valuable data on past summer climate¹⁻³. The thermal drilling technique used in previous studies precluded extending this record beyond a few hundred years. Recently, however, a 901-m deep ice core was mechanically drilled in southern Greenland. The excellent core quality and improved techniques for measuring melt features have resulted in an ~2,200 yr record with consistently high quality data. We report here that the temporal variations in melt feature abundance, expressed as annual melt per cent (AMP), provide proxy data on summer climate that can complement other records extending back thousands of years. The AMP profile indicates that relatively warm summers prevailed from about AD 950 until AD 1500 culminating in the thirteenth and fourteenth centuries. The periods from about 300 to 150 BC, AD 350 to 550, and AD 1550 to 1800 had relatively little summer melting.

Ice melt features form when some of the meltwater produced by summer warmth and solar radiation percolates downwards along isolated pipe-like, nearly vertical channels and spreads laterally on favoured layers. When this percolating meltwater refreezes, the vertical channels form ice glands and the laterally spreading units form ice lenses or layers in the snow. The resulting ice layer is deficient in air bubbles and is readily distinguishable from bubble-rich glacier ice formed by dry snow compaction. An exceptionally warm spell can produce ice features which extend downwards by percolation, along isolated channels, into the snow of several previous years. This can happen in regions which generally have little or no melting at the snow surface as exemplified during mid July 1954 in north-west Greenland⁴. Such an event could lead to the conclusion that two

or three successive years had abnormally warm summers, whereas all the icing formed during a single period which lasted for several days. The location where melt features will have the greatest climatic significance is high in the percolation facies where summer melting is common but deep percolation is minimal⁴. Dye 3 in southern Greenland (65°11'N; 43°50'W) is such a location.

In 1979 and 1980, members of the joint US-Danish-Swiss GISP recovered a continuous, mechanically-drilled ice core at Dye 3 between the depths of 82 and 901 m. Each core segment was cut longitudinally with a bandsaw and the exposed inner surface was planed smooth with a microtome knife. The ice core stratigraphy was viewed through the planar surface with the aid of transmitted fluorescent light, and ice crusts, wedges, layers and glands were recorded full scale on a graphic log. The depth, thickness, and type of each melt feature were summarized and entered into a computerized data bank. The percentage of melt features in each annual accumulation layer (AMP), calculated using ice equivalent lengths³, is thus independent of annual layer thinning by ice flow⁵. Vertical ice glands were excluded from the calculations due to their spurious distribution within the ice sheet. The age determinations for these calculations were based on the measured oxygen-isotope stratigraphy down to the 225 m depth which represents the year AD 1576 (W. Dansgaard, personal communication). Below this depth age estimates were based on a geophysical flow model (S. J. Johnsen, personal communication), and a reference horizon at 494 m representing the AD 934 Icelandic eruption (C. U. Hammer, personal communication). The error on these ages is estimated to be ±3%.

The AMP values obtained from the 1980 Dye 3 core are compared in Fig. 1 with the results of a stratigraphic study on the 373 m ice core recovered from Dye 3 in 1971 (ref. 2). The 1980 core AMP values are 30-yr running means and the 1971 core values have been smoothed by a non-recursive low-pass digital filter with a transition band from 0.0 to 0.03 cycles yr⁻¹ (ref. 2). For the period 1650-1850, the long term variations in the AMP values at Dye 3 are quite similar in the two cores, although the 1971 values are about 2% higher due to the inclusion of ice glands in the calculation. The apparent correlation between the two cores, located ~1 km from each other, suggests that the climatic signal of melt features may be easily interpreted in spite of the horizontal inhomogeneity of individual ice layers. The AMP peaks at about 1795, 1730 and 1655 are also present in the

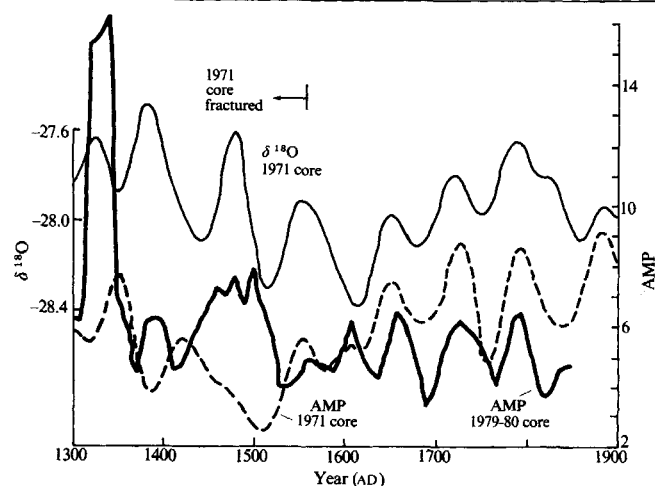


Fig. 1 AMP profiles at Dye 3, Greenland for the period AD 1300–1900. The 1979–80 ice core data are represented by the heavy line; the 1971 core profile² by the dashed line; and the $\delta^{18}\text{O}$ profile for the 1972 core² by the light line.

digitally-smoothed oxygen-isotope variations from the 1971 core, which are also shown in Fig. 1 (ref. 2). As $\delta^{18}\text{O}$ values reflect mean annual temperature, the correlation between the δ and AMP peaks suggests that short-term changes in summer climate in the period AD 1650–1850 paralleled changes in the average annual climate. The longer term $\delta^{18}\text{O}$ increase from 1650 to 1800 is not, however, reflected in the AMP profiles.

Before AD 1650, the correlation between the two AMP profiles is progressively worse. While the AMP values in the 1971 core consistently exceeded values in the upper part of the 1980 core, the averages are about equal for the period AD 1520–1650. Before AD 1520, the 1980 AMP values exceed those of 1971, and the two data sets show an anti-correlation. In this early time period, the 1971 AMP values are also negatively correlated with the $\delta^{18}\text{O}$ variations.

The apparent changes in the relative amounts of melting and the loss of correlation between the two AMP data sets can be attributed to differences in the quality of the two cores. The 1971 core, which was recovered with a thermal drill, contained thin, nested wafer-type fractures in all core below 230 m (AD 1560) which apparently obscured much of the visible stratigraphy. The Devon Ice Cap core, also thermally drilled, suffered similar wafering and obliteration of stratigraphy below the 150 m depth³. In contrast, the 1980 core was mechanically drilled and contains no wafer-type fractures. Because stratigraphy was viewed through the clear microtomed surface, it is unlikely that any significant features in the 1980 core were

missed. The loss of correlation between the two profiles may indicate that oscillations in the 1971 AMP profile below the 230 m depth are predominantly due to changes in the visibility of core stratigraphy rather than changes in the average degree of melting.

Many of the peaks in the 1980 AMP profile are also present in the δ -profile even in ice deposited before AD 1650. Both profiles show maxima at AD 1480, 1385 and 1325. The AD 1325 melt per cent peak reflects extremely warm summers in which the yearly AMP exceeded 20% in 7 of 32 yr between AD 1313 and 1344. Three of these summers are adjacent, however, and may be due to a single very warm period with heavy percolation. Although the peak heights in the δ and AMP profiles are not identical, seven of eight AMP peaks in Fig. 1 are coincident with δ -maxima, suggesting that changes in mean annual and summer climate were generally parallel before AD 1650 as well as after.

In addition to showing some correlation with the annual isotopic composition of local snowfall, the AMP profile at Dye 3 shows many similarities with proxy climate data from other regions. The entire 2,200 yr profile is shown in Fig. 2 with 20-yr mean AMP plotted against estimated age of deposition. The complete δ -curve for the 1971 core, digitally smoothed by Dansgaard *et al.*⁶ is also shown. Most of the trends in the Northern Hemisphere temperature index of Hammer *et al.*⁷, compiled back to AD 550 from various well-dated sources, including Greenland δ -profiles, are recognizable in the AMP profile. Relatively warm periods identified were AD 700–800, AD 1000–1300 ('mediaeval warmth'), and AD 1900–1950. The first two warm periods also have relatively high AMP values. Interestingly, the period of mediaeval warmth was not easily recognizable in the compiled Greenland ice core δ -profile⁷, but is readily apparent in the AMP record from AD 950 until perhaps as late as AD 1520. The 20-yr mean AMP for the period 1960–79, determined from Dye 3 shallow core analysis, is 8.0% compared with the deep core average of 5.6%. The relative warmth of recent summers indicated at Dye 3 was also found in the Devon Island ice core³.

Cooler periods in the Northern Hemisphere temperature index included AD 800–900, and AD 1350–1700 ('Little Ice Age') with a warmer period from AD 1500 to 1550. The AMP profile has corresponding minima at AD 820–940 and from AD 1540 to 1720, but the first portion of the 'Little Ice Age' from AD 1350 to 1500 is not pronounced in either the AMP or δ profiles. Although the period AD 1540–1720 stands out as the coldest in the last millenium in the AMP profile, two intervals within the preceding 1300 yr, 300–100 BC and AD 350–550, had even less summer melting. Much of the intervening period, 50 BC–AD 350 had a mild summer climate with AMP values similar to those in the early part of the mediaeval warmth.

The palaeoclimatic information provided by the AMP profile complements that obtained from the oxygen-isotope record. In addition to the fact that AMP values reflect summer rather than

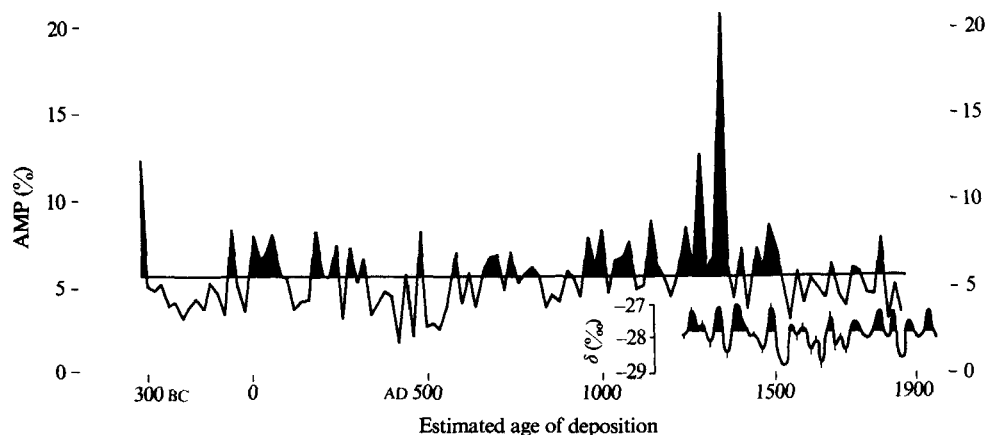


Fig. 2 AMP profile of the 1979–80 Dye 3 ice core. Also shown is the $\delta^{18}\text{O}$ profile for the 1971 Dye 3 core⁶. Above average values are shaded.

mean annual climate, several of the factors that may complicate the interpretation of an ice core δ -profile⁶ may not affect the AMP record. Among these factors are changes in the isotopic composition of seawater and the ratio of summer-to-winter precipitation. In addition, melt features are generally formed during periods without precipitation, while δ values are inherent properties of the snowfall. Finally, the oxygen-isotope variations in polar ice have undergone substantial diffusional smoothing⁸. In contrast, the melt features, once formed, remain as primary climatic information for at least 2,300 yr and perhaps thousands more.

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Ice age aerosol content from East Antarctic ice core samples and past wind strength

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Isotopic analysis of Antarctic deep cores provides valuable information on the Earth's past climate. Past atmospheric trace element contents of continental, volcanic, marine or other origins can also be reconstructed assuming that the chemical concentrations in the air and in the snow are well correlated^{1,2}. These atmospheric trace element contents, as well as atmospheric gas contents³, are important for climate reconstruction models because they influence the Earth's radiation balance^{4,5}. We consider here the possible link between aerosol content and climate which was investigated from the 905 deep Dome C ice core (East Antarctica) spanning some 32,000 yr (ref. 6). No evidence of major global or local volcanic activity was found, however, large marine and continental inputs (respectively 5 and 20 times higher than present) were observed at the end of the last glacial stage. We consider they reflect glacial age climate with stronger atmospheric circulation, enhanced aridity and faster aerosol transport towards the Antarctic continent.

110 ice sections, covering 5–10-yr periods, were processed for microparticle analysis (48 by Coulter Counter TA 2, 62 by a light scattering system^{7,8}) and 56 adjacent ice sections, covering 3–5-yr periods, were measured for trace element determinations (Al, Mn, V, Na, Cl by direct instrumental neutron activation analysis and Zn, after preconcentration, by atomic absorption).

To remove field contamination, the outer parts of the core sections were discarded. For particle measurements, this was done by recoring with a thermal probe and for chemical analysis, by rinsing the ice samples thoroughly with ultrapure water until

~60% of the original sample was melted off. We checked the contamination procedure by comparing our Holocene results with measurements performed on surface samples collected in hand dug pits in contamination-free conditions². Table 1 shows that the two data sets are in good agreement.

Figure 1 shows the variation of the trace element and microparticle concentrations in the Dome C core. The oxygen isotope profile of Lorius *et al.*⁶ is used as a chronological and climatic reference. Four main isotopic stages can be distinguished: the Holocene stage (stage 1), the isotopic transition (stage 2), the stage with minimum $\delta^{18}\text{O}$ values (stage 3) and the earlier stage (stage 4). In spite of discrepancies concerning the age of the deepest part of the core^{6,9}, stage 3 probably includes the last glacial maximum ($\approx 18,000$ yr BP (ref. 10)). Table 1 gives our main results and those obtained on 51 adjacent samples (L. G. Thompson, personal communication) for the four different isotopic stages. Taking Al as the reference element, the crustal enrichment factors of Mn and V are close to 1 (the crustal enrichment factor of element X is given by $(\text{X}/\text{Al})_{\text{snow}}/(\text{X}/\text{Al})_{\text{crust}}$ with the crustal composition given by ref. 11). Al correlates well with Mn (correlation coefficient: $\rho = 0.92$) with V ($\rho = 0.98$) and with the light-scattered signal ($\rho = 0.80$) which is related to the microparticle concentration. Scanning electron microscope (SEM) and energy dispersive X-ray (EDAX) analysis performed on 399 insoluble microparticles randomly chosen from samples covering all the time periods demonstrated that 90% of the particles are various silico-aluminates and ~10% are quartz²⁵. Thus Al and microparticles must both be representative of the terrestrial contribution. Cl and Na (with a Cl/Na ratio close to the bulk seawater value) are most likely of marine origin. Zn has been proposed as a good indicator of volcanic activity for Antarctic snow samples¹².

The highest terrestrial concentrations are observed during stage 3 with mean values 21, 23 and 8 times higher than during stage 1 (for Al, light-scattered signal and microparticle number concentrations respectively). The lower ratio obtained with the Coulter Counter may be due to the sedimentation of particles during measurements or to the influence of large particles (statistically sparse but having a large influence on the total mass). Marine input shows a similar trend but with a lower ratio of decrease from stage 3 to 1 (Cl:3.8, Na: 5.7). Na and Cl decrease during stage 2 and reach their Holocene values at the end of stage 2 a few thousand years after the terrestrial contribution. The Dome C results compare very well with those obtained at other sites in North and South Pole regions (Table 2) (refs 13–15, and M. Briat, personal communication). However, the magnitude of the concentration decrease from glacial to Holocene depends on the site; in Antarctica the maximum ratio occurs on the central plateau (Vostok and Dome C). These large global and roughly synchronous variations may tentatively be explained by several mechanisms, involving changes in source or mode of transport. The decrease in Dome C snow accumulation rate proposed for stage 3^{6,9} does not exceed 50% and therefore cannot explain the observed concentration changes.

In the last glacial part of the Dome C core, we found no evidence of local volcanism such as the ash bands observed in the Byrd core¹⁶ that were attributed to Mary Byrd Land volcanoes¹⁷, whose active period reached a maximum before Dome C concentrations significantly increased. Although one volcanic glass shard was found at a depth of 726 m (ref. 18), qualitative results obtained using the EDAX microprobe show that only 10% of the 399 studied particles are close in composition to this glass shard and that the relative abundance of such particles is lower during rather than after stages 3 and 4⁵⁵. Moreover, short-term microparticle variations¹⁹ show continuously higher background values during stage 3 and high volcanic activity would have had to have been unceasing over thousands of years to enhance the atmospheric aerosol loading without any return to lower concentrations. This is, however, statistically improbable⁴ and does not agree with late Pleistocene volcanic data^{20,21}. Furthermore, volcanic aerosol generally contains large

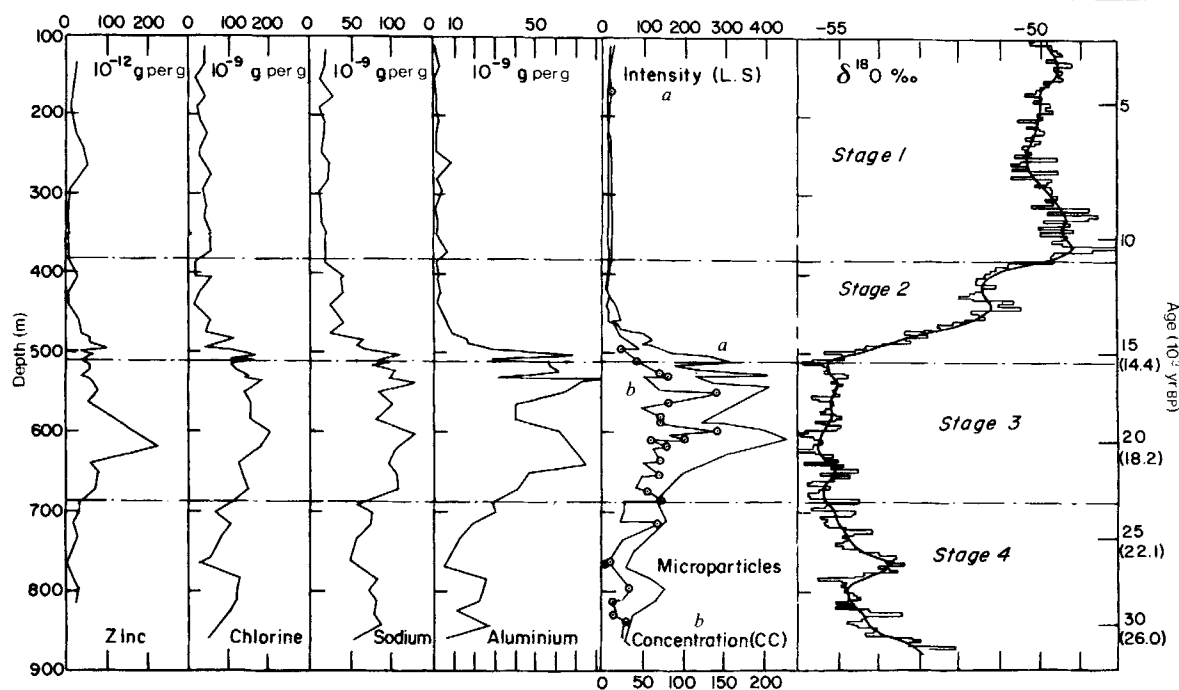


Fig. 1 Oxygen isotope, trace element and microparticle concentrations along the Dome C ice core: *a*, microparticle results from light scattering (LS) in relative units; *b*, results from Coulter Counter (CC) in 10^3 part. $r > 0.4 \mu\text{m}$ per g of snow—the chronology and the isotopic profile with individual stages are after ref. 6. Ages are estimated from a simple ice flow model assuming a variable or constant (numbers in brackets) rate of accumulation.

amounts of sulphur compounds, principally H_2SO_4 (refs 22, 23) and no significant changes in the SO_4 concentration (R. Delmas, personal communication) nor in acidity²⁴ were found at Dome C during the glacial stage. This agrees with the SO_4 measurements from the Byrd core¹⁴.

The average Zn concentrations are higher during stage 3 (Fig. 1). Nevertheless, the crustal enrichment factor of Zn (reference element: Al), which is rather high during stage 1 and reaches values higher than 100 in surface samples corresponding to time periods including major volcanic events¹², becomes close to 1 during the glacial period (stage 3).

All this information indicates that the high observed terrestrial input enhancement is not linked to local or global

volcanism and is more likely of continental origin. This conclusion is supported by results from Byrd station¹⁴ and from the Northern Hemisphere¹³⁻¹⁵.

The Southern Hemisphere deserts are thought to be prime sources of present continental input to Antarctica²⁵. Geomorphological features and deep-sea core studies indicate that during last glacial maximum the Southern Hemisphere continents were drier²⁶⁻²⁹ and that active sand deserts between 30°N and 30°S were five times larger than today²⁹. Lastly, continental shelves exposed by the lowering of sea level were an additional source of atmospheric dust. Larger source areas also coincided with stronger winds near the ground in many parts of the world, as shown by eolian features including ventifacts, loess

Table 1 Average for microparticles and trace elements for the different isotopic stages in the Dome C ice core

Isotope stage	Depth (m)	Light scattering (signal in relative units)	Particles $> 0.4 \mu\text{m}$ in radius (no. per g of snow)	Concentrations (10^{-9} g per g)			Crustal enrichment factor			Cl/Na
				Al	Na	Cl	V	Mn	Zn	
1 includes Holocene period	0-381	13 ± 4 (9)	$6,800 \pm 4,000^*$ (23) $7,200 \pm 3,300^\dagger$ (12)	2.7 ± 2.3 (15)	18 ± 5 (15)	39 ± 12 (15)	‡	$1.4 \pm .4$ (15)	12 ± 10 (10)	$2.2 \pm .7$ (15)
2 includes isotopic transition	381-510	93 ± 108 (26)	$13,600 \pm 16,400^*$ (7) $16,800 \pm 12,300^\dagger$	21 ± 21 (15)	59 ± 30 (15)	73 ± 48 (15)	$1.4 \pm .4$ (10)	$1.5 \pm .5$ (15)	3.7 ± 2.8 (13)	$1.2 \pm .7$ (15)
3 includes glacial maximum	510-687	298 ± 89 (18)	$55,100 \pm 16,600^*$ (10) $86,600 \pm 30,100^\dagger$ (15)	57 ± 15 (14)	102 ± 15 (14)	150 ± 25 (14)	$1.2 \pm .2$ (14)	$1.2 \pm .2$ (14)	$1.4 \pm .8$ (11)	$1.5 \pm .2$ (14)
4	687-880	96 ± 39 (9)	$22,800 \pm 7,300^*$ (11) $23,000 \pm 22,000^\dagger$ (8)	18 ± 9 (12)	69 ± 13 (12)	88 ± 31 (12)	$1.2 \pm .7$ (12)	$1.2 \pm .4$ (12)	$1.4 \pm .8$ (7)	$1.3 \pm .4$ (12)
Present time	snow 5 m-pit	5.4 ± 4.8	$6,000 \pm 2,500$	$1.7 \pm .9\%$	$21 \pm 4\%$			$1.3 \pm 1.4\%$	$35 \pm 15\%$	

For each period, the mean value, standard deviation and number of samples analysed (in brackets) are given. * From ref. 19 and L. G. Thompson, personal communication. † This study. ‡ Vanadium below detection limit. § From ref. 2.

Table 2 Ratio between late-glacial stage and Holocene concentrations obtained from different ice cores in both Southern and Northern Hemispheres

Site	Altitude (m)	Distance from coast (km)	Microparticles	Al	Na
Dome C (74°39'S, 124°10'E)	3,240	1,070	8* 12† 23‡	21	5.7
Vostok (78°30'S, 106°54'E)	3,480	1,294	—	24§	3.4§
Byrd (80°01'S, 119°31'W)	1,530	670	2.5	7.3¶	1.7¶
D10 (66°40'S, 140°01'E)	260	5	—	7.3§	3.1§
Camp Century (77°10'N, 61°08'W)	1,890	200	9.5*	11¶	3.0¶
Devon Island (75°22'N, 82°27'W)	1,800	30	20–30**	—	—

* From ref. 19, and L. G. Thompson (personal communication) with the chronology from ref. 6. † This study: Coulter Counter results. ‡ This study: light scattered signal. § From M. B. (unpublished data): || From ref. 13 with the chronology from ref. 52. ¶ From ref. 14. * From ref. 13 with the chronology from ref. 53. ** From ref. 15.

transportation and active sand dunes^{27,30,31}. Aeolian deflation should have therefore been enhanced. If long-range dust transport is taken into account^{32–35}, intertropical glacial aridity in both hemispheres³⁶ together with stronger wind over arid continents may have been sufficient to produce enough dust to have a worldwide impact and therefore substantially to increase the aerosol content over Antarctica.

It has been demonstrated^{37,38} that the stronger the Trade Wind, the higher the proportion of large particles in Sahara dust-laden air masses over the Atlantic ocean. Dome C results show a shift in the microparticle volume distribution (Fig. 2) during the ice age (particles as large as 5 µm in radius being present during but not after stage 3). This suggests a more vigorous circulation from the tropical zone towards Antarctica during the glacial age perhaps due to a stronger meridional circulation. Both meridional and zonal circulations are driven by the baroclinic eddy process and therefore seem to be linked to the pole–equator temperature gradient^{39–41} which was greater than at present during the last glacial age, as shown by ice and deep-sea core studies^{42,43}. Our results are compatible with a stronger meridional temperature gradient.

The continental input decreases during stage 2 up to 440 m where it stabilizes at the stage 1 value. This is before the end of the isotopic transition (381 m) and according to available dating results^{6,9}, 440 m corresponds to ice deposited between 12,000 and 13,500 yr ago. This period corresponds closely to the end of the aridity maximum in Africa²⁸ and the stabilizing of sand dunes in Australian desert owing to the “relaxation of stress from a vigorous circulation”²⁷.

Wind activity should also have been enhanced above the sea surface during the ice age. Climatic reconstruction by Hays *et al.*⁴⁴ indicates that the limit of sea ice was 500–1,000 km farther north at 18,000 yr BP. In spite of the greater extent of sea ice, the sea salt content in the aerosol over central Antarctica was higher during glacial time. This may tentatively be explained by stronger aerosol production, which is mainly driven by the wind velocity at the sea surface^{45–47}. Furthermore, the Cl/Na ratio was slightly lower than the bulk sea ratio of 1.8 during the glacial stage (even after removing the terrestrial component of Na) and very close to those presently observed near the coast⁵⁴. This suggests a shorter transport time in 18,000 yr BP than at present. No Cl data are presently available for other Antarctic sites for the glacial period. Over the past 32,000 yr Na shows a better correlation with isotopes, representative of local climatic conditions, than Al ($\rho = 0.90$ and $\rho = 0.64$ for 56 points respectively). This suggests that Na is more directly linked with storminess around Antarctica. Present Na snow concentrations are greatest during winter or early spring^{48,49} in East Antarctica (in spite of greater distance from open sea), probably owing to increased storm activity. Present day winter and past glacial circulation mean conditions seem to cause the same qualitative effect and are therefore probably similar. Furthermore, large-scale circulation and wind near the sea surface may be related and both

have a positive influence on the aerosol flux over Antarctica.

Manabe's climatic reconstruction of the ice age period⁵⁰ predicts higher meridional fluxes and a circulation which agrees with some of our conclusions.

An increase in wind strength over continental regions may be deduced from $F \propto u^3$ (ref. 51) where F is the vertical aerosol flux and u the friction velocity which corresponds roughly to the wind speed near the ground. Assuming that tropical arid zones were five times bigger²⁹, wind speeds 1.3–1.6 times higher than present are sufficient to increase the atmospheric dust load by a factor of 10–20. Similarly, when considering the relation $\log c = au + b$ observed above the sea surface^{45–47} where c is the sea salt-derived aerosol atmospheric concentration, u the wind speed and a varies between 0.16 and 0.25; an increase in c by a factor of 4 requires a 5–8 m s^{−1} increase in wind speed. If 10 m s^{−1} is taken as an average value for the subAntarctic region (Kerguelen Island (data from Meteorologie Nationale,

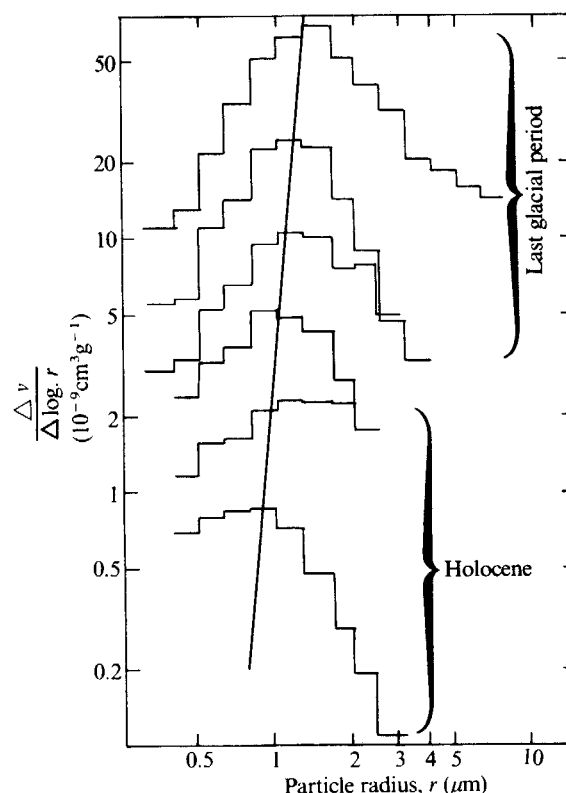


Fig. 2 Typical volume distribution for insoluble microparticles from the Holocene and the last glacial periods.

Boulogne)), then the wind speed was 1.5–1.8 times higher during the last ice age. However, such estimates must be considered as maxima as change in large-scale transport is not taken into account.

We suggest that ice age marine and continental input increases observed in Antarctica are mainly linked to enhanced wind activity and aridity and are therefore a consequence of glacial climatic conditions. However, the aerosol effect on the Earth's radiation balance and therefore on global temperature, although not yet understood, should probably be taken into account in climate modelling because the decrease in atmospheric aerosol load may have had a positive feedback effect on temperature during the transition from Pleistocene to Holocene.

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Palaeotidal speeds and ranges estimated from cross-bedding sets with mud drapes

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Tidally formed cross-bedding sets in which mud drapes are spaced out among sandy foresets occur in Cretaceous^{1,4}, Eocene⁵ and Holocene⁶ sands deposited subtidally by strongly asymmetrical sand waves. A striking feature of some sets^{4,6} is that the horizontal drape spacing, measured in the direction of sand-wave advance, varies cyclically on a time scale related to the spring-neap tidal cycle as indicated by the numbers of drapes. Each drape forms part of a distinctive lithological couplet, consisting of a group of sandy foresets recording avalanching on the steeper side of the sand wave, followed by the drape itself, typically divided into upper and lower parts by a centrally placed silt-sand parting. The drapes have been assigned to slack water, but the groups of sandy foresets to episodes of vigorous flow, the combination suggesting a quantitative model based on the tide. Here I show how the peak speeds and ranges of ancient tides can be estimated by combining an empirical bedload transport function with appropriate measurements from such cross-bedding sets.

Visser⁶ and Allen⁴ independently explained the lithological couplet by the action of a strongly asymmetrical tide, in which one stream (flood or ebb) flowed much more strongly than the other. The sandy foresets represent bedload transport beneath the dominant stream, the lower part of the drape the slack water following this stream, the silt-sand parting the weak bed-material transport due to the subordinate stream, and the upper portion of the drape the slack water preceding the secondmost dominant stream. Hence the sequence of drape spacings records a pattern of changing strength of flow which can be interpreted by introducing an appropriate bedload transport function. Knowing the flow strength, and given an independent estimate of flow depth, the tidal range and its variations can be calculated. Previous attempts to estimate the ranges of past tides^{7,8} have suffered from a reliance on generalized and ambiguous criteria.

We require formulae for (1) the flow speed of the dominant stream over a tidal cycle, (2) the bedload transport rate due to that stream, and (3) the sand-wave celerity in relation to the bedload transport rate. The speed $U(t)$ of the dominant stream is taken to be

$$U(t) = U_m \sin\left(\frac{2\pi t}{T}\right), 0 < t < T/2 \quad (1)$$

where U_m is the maximum speed, t time and T the tidal period (12.42 h for semidiurnal tide; ~24 h for diurnal tide). As there is no universally accepted way of representing the spatially averaged bedload transport rate $J(U)$, we choose

$$J(U) = k(U - U_{ces})^3, \quad U \geq U_{ces} \quad (2)$$

where J is the dry mass per unit width and time, k an empirically determined dimensional sediment transport coefficient and U_{ces} the tidal speed for sediment entrainment^{2,9,10}. The cube exponent conforms with theory^{9,11} and experiment^{12,13}, provided that only moderate to high transport stages are considered. As most sand waves carry dunes, thereby implying such stages, k is evaluated below for the dune regime. The speed of sand-wave advance $V(J)$ is

$$V(J) = \frac{2J}{H\gamma} \quad (3)$$

where H is the sand-wave height and γ the sediment dry-bulk density, a triangular sand-wave cross-section being assumed⁹.

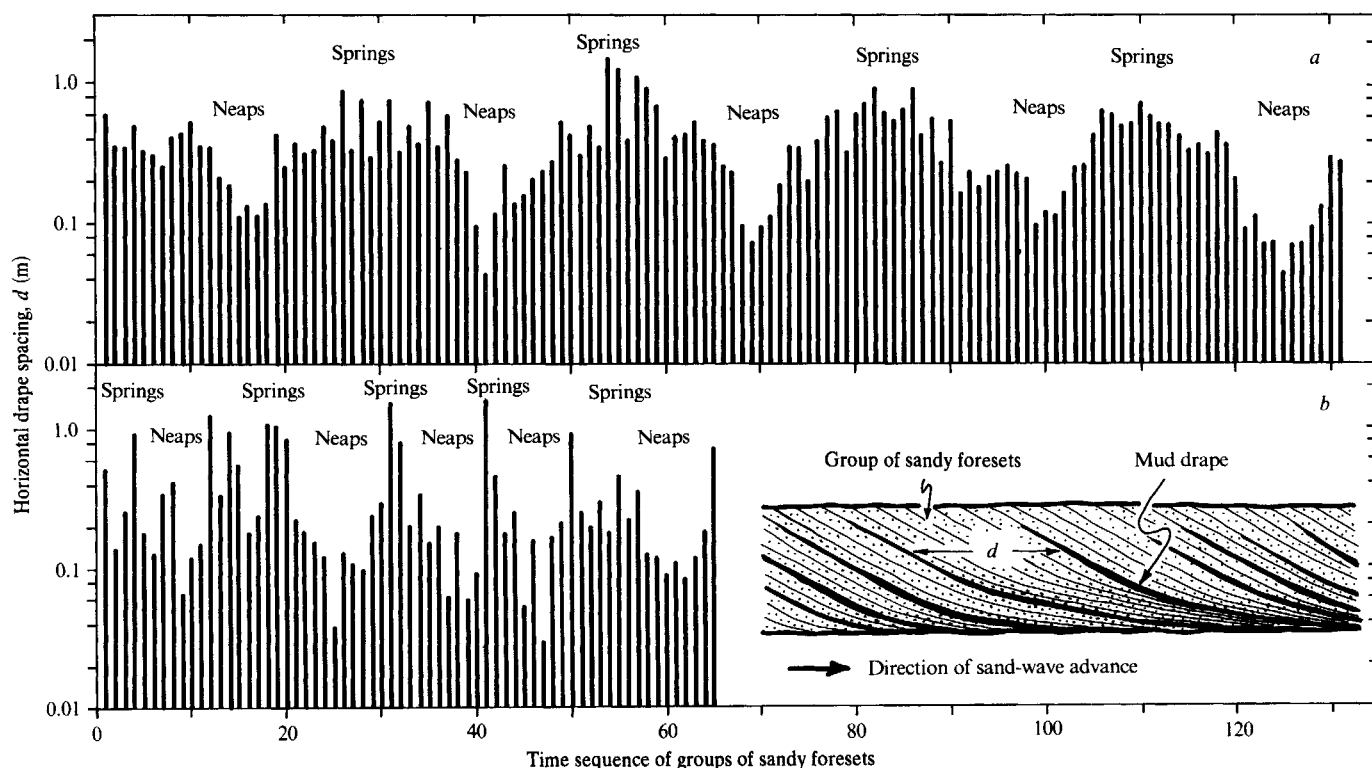


Fig. 1 Drape spacing as a function of time sequence of groups of sandy foresets in cross-bedding sets with mud drapes. *a*, Holocene set, Oosterschelde (ref. 6, Fig. 4, with bundle thickness corrected to horizontal spacing). *b*, Cretaceous set, Folkestone Beds, Midhurst (ref. 4, Fig. 1, with horizontal spacing corrected to true value).

By substituting equation (2) into equation (3) and integrating, the drape spacing, d , is obtained as

$$d = \frac{2k}{H\gamma} \int_{t_1}^{t_2} [U(t) - U_{ces}]^2 dt, \quad U(t) \geq U_{ces} \quad (4)$$

where t_1 and t_2 are the times during flow of the dominant stream when $U(t) = U_{ces}$.

The model can be used in two ways to interpret Cretaceous and Holocene cross-bedding sets. Visser⁶ measured 131 drape spacings (Fig. 1a) from a set in the Oosterschelde (Netherlands), thought to have accumulated 200–300 yr ago 10–15 m below sea level. The lengths of the cycles of spacings implied a semi-diurnal tide, slightly more than 2 months of sand-wave advance being covered. Allen⁴ measured 65 drape spacings (Fig. 1b) from a set in the shallow-marine Folkestone Beds (Cretaceous) near Midhurst (Sussex), the precise water depth being unknown. The lengths of his cycles of spacings suggested a diurnal tide (sand omission at times) and about 2.5 months of sand-wave activity.

How do the peak speed and range for the dominant tide at springs compare with the same quantities in neap conditions? Tidal theory¹⁴ gives as a first approximation

$$\frac{U_{ms}}{U_{mn}} = \frac{a_s}{a_n} \quad (5)$$

where U_{ms} and U_{mn} are the peak speeds and a_s and a_n the amplitudes (assumed to equal one-half of the corresponding ranges) respectively at springs and neaps. We can provisionally neglect U_{ces} in equation (2) provided that U_{ces} is substantially less than U_{ms} and U_{mn} , on the grounds that a ratio is under consideration. Equation (4) then becomes

$$\frac{U_{ms}}{U_{mn}} = \left(\frac{d_s}{d_n}\right)^{1/3} \quad (6)$$

where d_s and d_n are characteristic drape spacings in spring and neap conditions, respectively. The neglect of U_{ces} in equation (2) is perhaps acceptable for the Holocene set ($U_{ces} \approx 0.22 \text{ m s}^{-1}$) but certainly more limiting in the Cretaceous case ($U_{ces} \approx$

0.30 m s^{-1}) at least using values estimated experimentally¹³. The characteristic spacings listed in Table 1 are the means of assembled spacings taken four at a time from each of Visser's spring and neap tides (Fig. 1a) and two at a time from Allen's (Fig. 1b) neaps and springs, to smooth out storm effects, tidal inequalities and interference between neighbouring sand waves. The calculated peak speed and amplitude ratios in each case approach 2. The amplitude ratio of 1.87 from Visser's data agrees with 1981 tidal predictions for Flushing (Vlissingen), only 23 km from his site. The tabulated overall mean ratio is 1.52, ranging from ~ 1.3 near a solstice to ~ 2.0 near an equinox¹⁵.

What were the absolute tidal speeds and ranges? Values for k , H , γ and U_{ces} are now required for equation (4). Laboratory data¹³ suggest the values for k (dune height and speed) and U_{ces} in Table 1 (values not in parentheses) given the reported grain sizes of the sets. Sand-wave height is presumed to have been 50% larger than the thickness H_{set} of the preserved sets, on comparison with migrating current ripples^{16–18}. However, as current ripples are more strongly three-dimensional than sand

Table 1 Comparison of values for Cretaceous (Folkestone Beds) and Holocene (Oosterschelde) cross-bedding sets

Quantity	Cretaceous set ⁴	Holocene set ⁶
d_s (m)	0.875 (mean of 10)	0.699 (mean of 16)
d_n (m)	0.088 (mean of 10)	0.107 (mean of 20)
d_s/d_n	9.94	6.53
U_{ms}/U_{mn} , a_s/a_n [equations (5), (6)]	2.15	1.87
k ($\text{kg s}^{-2} \text{ m}^{-4}$)	2.19 (0.85)	1.21 (0.59)
U_{ces} (m s^{-1})	0.30	0.22
H_{set} (m)	1.88	~ 2
γ (kg m^{-3})	1,600	1,600
U_{ms} (m s^{-1}) [equation (4)]	0.70 (0.83)	0.78 (0.91)
U_{ms}/U_{mn} [equation (4)]	1.40 (1.48)	1.47 (1.49)
Water depth (m)	—	12.5
a_s (m) [equation (4)]	—	0.96 (1.03)
a_s/a_n [equation (4)]	—	1.47 (1.49)

waves, the height of the parent bedforms may have significantly exceeded this estimate. A porosity of 40% is plausible for the loosely packed avalanched sand¹⁹, whence $\gamma = 1,600 \text{ kg m}^{-3}$. The calculated tidal peak speeds and amplitudes¹⁴ are plausible (Table 1, values not in parentheses), suggesting that the general tidal model is apt, but seem low by a factor of ~ 2 , judging from the Holocene case of the overall mean tidal ranges (springs = 4.4 m, neaps = 2.9 m) predicted at Flushing¹⁵. Equation (2) apparently overestimates the bedload transport rate when k is based on laboratory experiments, possibly because these were performed at relatively very shallow flow depths. Although Colby's²⁰ data on the total sand transport of four moderate to large rivers do not equal in quality the laboratory results, they give estimates for k (Table 1, values in parentheses) significantly lower than the experimental ones, when empirically adjusted to bedload alone¹³. The peak tidal speeds and ranges are better (Table 1, values in parentheses) but still low, indicating overestimation of bedload transport. In the tidal case, the widespread

deposition of mud and its persistence on the bed from one tide to another may significantly inhibit sand transport during the episodes of dominant current, by armouring the bed (U_{ces} raised) and by reducing the area from which sand can be scoured (flow undercharged with sand). Underestimation of the sand-wave height may also have affected the results.

The above simple model helps us to quantify past cross-bedded tidal sediments. It strengthens the tidal interpretation, and provides crude estimates of the ratios of spring and neap peak tidal speeds and ranges, together with very rough absolute values for range and speed. Its imperfections are attributable to (1) the simplicity of the flow and sediment transport functions used, (2) poor understanding of sediment transport, and (3) possible differences between sand transport in tidal conditions, when mud can accumulate extensively, and in rivers and flumes. The third factor may prove a permanent limitation, in which case the model would be better applied to cross-bedding sets devoid of mud drapes.

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Predator evasion as an explanation of diurnal vertical migration by zooplankton

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Diurnal vertical migrations of planktonic crustaceans are widespread phenomena both in freshwater lakes and in the sea. The adaptive significance of this behaviour is unclear. This movement from the warm surface layers, which provide abundant food, to colder, deeper water with its poor food supply must place the migrating animals at a disadvantage compared with individuals remaining near the surface. The reduced availability of food and the energy consumed in migration result in reduced growth and reproductive capacity of migrating individuals. Moreover, the development time of the eggs carried in the brood pouch or in egg sacs is prolonged in cold water and the birth rate is reduced. We have studied vertical migration in a deep lake and report here that two very similar *Daphnia* species have different strategies. *Daphnia hyalina* shows a pronounced diurnal migration whereas *Daphnia galeata* remains near the surface. Although the non-migrating *D. galeata* has a much higher birth rate than *D. hyalina*, the latter is numerically dominant, as *D. galeata* suffers a high mortality near the surface. These results support the hypothesis that predator avoidance is one of the most important factors in vertical migration.

As migration behaviour has evolved in different taxonomic groups in spite of a probable reduction in fitness, there must be some selective pressure favouring it and providing a higher overall fitness to migratory populations. Many hypotheses have been put forward to explain this selective pressure, including the avoidance of optically orientating predators¹ and metabolic advantages of alterations in temperature² or food³. To determine the distribution patterns of zooplankton and the changes in food levels and temperature to which these animals are exposed, we carried out an intensive study of diurnal migration and environmental parameters in Lake Constance (southern FRG). This lake is mesotrophic with an area of 476 km², a maximum depth of 252 m and a mean depth of 100 m.

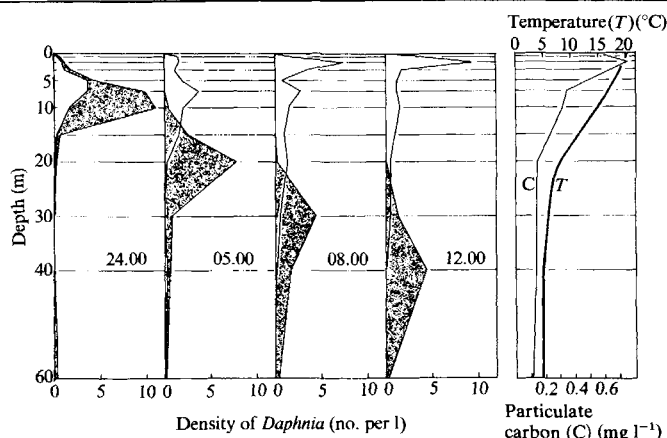


Fig. 1 Typical examples of the different diurnal vertical migrations of *D. galeata* (open area) and *D. hyalina* (shaded area) in Lake Constance, July 1977. The left panel shows four of seven measured depth distributions of the populations at different times of day. The right panel illustrates the temperature and food gradients in the water column. Only that fraction of particulate carbon $< 30 \mu\text{m}$ is considered as filterable food. Such distributions were recorded monthly from February 1977 to April 1978. Zooplankton samples were taken at 11 different depths (vertical lines) using a 30-l zooplankton trap every 4 h for 28 h. Temperature and particulate carbon were measured 4 times a day; as there were only minor diurnal variations the values were averaged.

When following the migratory pattern during an annual cycle, we discovered interesting differences between the populations of *D. galeata* and *D. hyalina*, which are of a similar maximum size (1.7–2.0 mm) and general morphology. A typical example of the difference in behaviour is shown in Fig. 1. During the night both populations are in the upper layers of water, whereas in the morning *D. hyalina* moves downwards so that the two populations are clearly separated during the day. In the late afternoon *D. hyalina* migrates back to the upper layers so that the two populations again overlap during the night. As there are substantial vertical gradients of temperature and food the populations are exposed to different environmental conditions. Most of the *D. galeata* population stays in warm water in conditions of abundant food, whereas *D. hyalina* lives at 5 °C for a considerable part of the day. The concentration of food in

Table 1 Seasonal changes in the population dynamics and mean environmental conditions to which migrating *Daphnia hyalina* and non-migrating *Daphnia galeata* are exposed

Month	Species	Abundance (no. per m ²)	Mean concentration of food (mg C l ⁻¹)	Mean temperature (°C)	No. of eggs per adult	Egg development time (days)	Instantaneous birth rate
May	<i>D. galeata</i>	41,514	0.705	9.3	6.2	8.9	0.101
	<i>D. hyalina</i>	9,887	0.633	8.9	4.2	9.6	0.079
June	<i>D. galeata</i>	342,006	0.080	9.5	0.05	9.1	0.002
	<i>D. hyalina</i>	600,133	0.075	7.4	0.2	13.3	0.003
July	<i>D. galeata</i>	639,906	0.143	14.6	3.5	6.8	0.122
	<i>D. hyalina</i>	220,610	0.096	7.4	0.64	13.1	0.024
August	<i>D. galeata</i>	38,207	0.336	14.2	7.1	8.8	0.147
	<i>D. hyalina</i>	91,500	0.155	7.1	3.68	14.5	0.055
September	<i>D. galeata</i>	154,789	0.184	12.4	2.20	9.1	0.056
	<i>D. hyalina</i>	272,212	0.107	6.7	1.01	15.4	0.029
October	<i>D. galeata</i>	14,155	0.161	10.5	1.90	7.1	0.105
	<i>D. hyalina</i>	30,580	0.122	8.3	0.88	8.9	0.039
November	<i>D. galeata</i>	18,634	0.144	9.9	2.60	7.9	0.057
	<i>D. hyalina</i>	123,044	0.116	8.3	2.60	9.9	0.056

Population size was estimated by integrating the depth distribution of numbers from 0 to 60 m and taking the average of six estimates per day. Particulate carbon smaller than 30 µm was considered as food. A weighted average of the food concentration was calculated for each sampling time using the concentrations at the different depths and the corresponding numbers of animals. The mean concentration of food to which the animals were exposed during the whole day was calculated for the 24-h series; the same procedure was followed for the mean temperature. Animals 1.3 mm long were considered to be adults. This was the minimum size at which the first eggs appeared in the brood pouch. Egg development time was calculated from the mean temperature⁷. It was assumed that temperature fluctuations do not significantly affect the speed of development. Instantaneous birth rates were calculated from the number of eggs and the egg development times⁷.

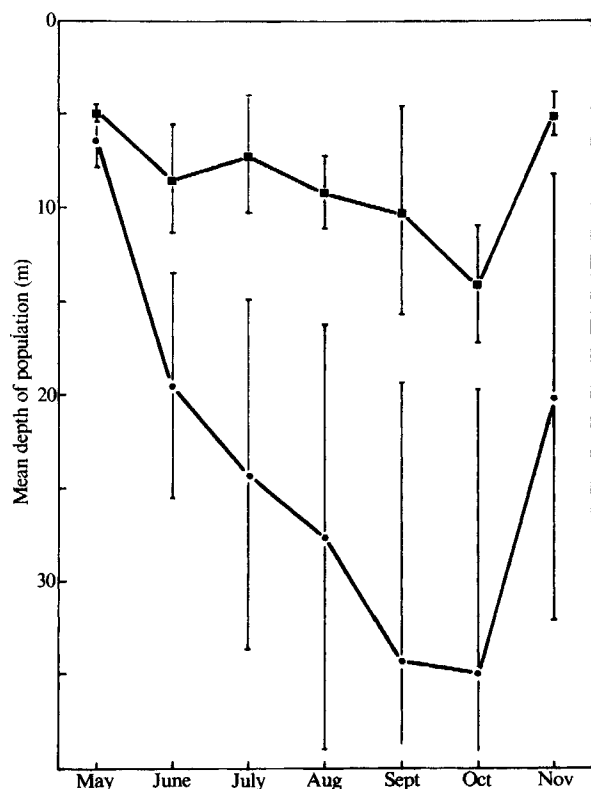


Fig. 2 Seasonal variations in the vertical migrations of the two daphnid species. The mean depth of the whole population was calculated from the distributions at different times of the day as illustrated in Fig. 1. Points represent the average of six mean vertical positions per day, with standard deviations calculated from these six positions, which give a measure of the daily amplitude of migration. From May to October, depth and amplitude of the vertical migration of *D. hyalina* (●) increase continuously, whereas there is only a slight seasonal effect in *D. galeata* (■).

Differences between the two populations change during the annual cycle (Fig. 2); during early spring and winter there is no migration at all. Differential behaviour starts in June, after which the mean depth of the *D. hyalina* population is much greater than that of *D. galeata*, as is the daily amplitude of migration. In October, the *Daphnia* reach their maximum depth and amplitude of migration, and then the population returns to the initial state in winter. This behaviour cannot be explained by a simple avoidance reaction to the warm water, as in laboratory experiments both species grow and reproduce very well at a constant temperature of 20 °C. In these conditions *D. galeata* has only a slightly higher reproductive capacity than *D. hyalina*. We also studied both species simultaneously in simulated migratory conditions. In a flow-through system they were exposed to 20 °C with food during the night, and to 8 °C without food during the day. In this case *D. hyalina* produced more young than did *D. galeata*.

The different migratory pattern has severe consequences for the population dynamics of the two species (Table 1). Because of the better food conditions, the number of eggs per adult in *D. galeata* is higher than in *D. hyalina*. Moreover, the eggs of *D. galeata* develop much faster due to the higher temperature to which they are exposed⁵. As a result the instantaneous birth rate of *D. galeata* is considerably higher than that of *D. hyalina* during the period of different diurnal vertical migration. From these data, the *D. galeata* population would be expected to grow much faster than that of *D. hyalina*; the former should theoretically reach such large numbers that it eventually outcompetes the latter. Surprisingly this is not the case. Numbers of the migrating *D. hyalina* are higher almost all summer, with the exception of July. Thus, there must be severe mortality of *D. galeata*, due to predators or some other factor near the surface. In the case of optically orientating predators such as fish, this predation pressure must be reduced during the night when *D. hyalina* feeds in the upper layers. In fact, analyses of the gut contents of certain whitefish (*Coregonus wartmanni*) and perch (*Perca fluviatilis*) showed that the fish ate nine times as many *D. galeata* as *D. hyalina*.

Thus we conclude that by remaining in optimal conditions, *D. galeata* is able to compensate for high mortality by high propagation; by migrating into deeper waters during the day, *D. hyalina* reduces its mortality but must tolerate low birth rates. The results of the laboratory experiments with fluctuating temperature and food conditions indicate that the latter strategy would be less effective for *D. galeata*.

deep water can hardly meet the minimum energy demands of the daphnids although at the lower temperatures that prevail there, the metabolic rate is also reduced. However, the feeding rate is lower in these conditions. Food particles (smaller than 30 µm) must provide a minimum concentration of 0.1 mg carbon per l for the animals to reproduce⁴.

As daphnids reproduce parthenogenetically during the summer a population can be considered as an assembly of many clones⁶. Therefore the increase in depth during the day and amplitude of migration (Fig. 2) might be generated by differential response of individuals to changing environmental conditions or by selection pressures eliminating non-migrating animals from the population, as long as the predators are active. When the pressure of predators ceases in autumn, the remaining non-migrating animals would be favoured so that the variation of the population would become re-established. These hypotheses remain to be tested by genetic studies.

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Visual orientation behaviour of flies after selective laser beam ablation of interneurons

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The use of a laser microbeam has allowed us to ablate specific small groups of cells deep in the larval brain of the housefly *Musca domestica*, without damage to overlying tissues. The ablation results in various specific alterations in brain structure and behaviour of adult flies. In the experiments described here, a set of large horizontal and vertical neurones on one side of the adult optic lobes was ablated. These neurones are known to be directionally motion sensitive and are thought to play an essential part in visual orientation of the fly^{1–8}. However, visually guided orientation behaviour towards single objects by flies lacking the large horizontal and vertical neurones on one side was not significantly different from the behaviour of normal flies, although the response to moving gratings was reduced on the ablated side. Therefore we suggest that other sets of nerve cells are involved in the information processing of single moving objects, whereas the horizontal and vertical cells seem to be implicated in ordinary optomotor flight stabilization.

At least 12 different classes of directionally motion-sensitive large cell have been identified anatomically and electrophysiologically in the posterior lobula plate of the optic lobes^{3,9}. In the flies *Calliphora* and *Phaenicia*, some of these cells respond to horizontal motion^{2,4}, others to vertical motion^{2,6}. Among these are two sets of larger ipsilateral neurones: the H-cells (three neurones), which respond mainly to horizontal motion and are believed to be involved in optomotor turning responses as well as in visual orientation towards single objects, for example, fixation and tracking^{1,4,7,8,10}; and V-cells (8–11 neurones), which respond mainly to vertical motion, and are considered important in thrust, lift and landing responses^{4–6,10} (for review, see refs 10–12). Here we investigated the role of these cells in visual orientation by examining the behaviour of the fly after their ablation.

Elimination of the cells in adult flies proved to be technically difficult; therefore we decided to ablate their precursors in the larval brain. The development of the optic lobe neuropile starts halfway through the third larval instar¹³, but lobula-plate neuropile differentiation starts later, probably in pupal stages^{13,14}. We therefore decided to perform the ablations in larvae before the third instar.

The primary aim of the experiment was to determine and progressively tune the parameters required selectively to affect the cells of the lobula plate. Fly larvae were anaesthetized briefly with ether and brought into the light path of a microscope incorporated in a pulse dye-laser microbeam surgery unit (G.D., D.R.N. and H. S. Seyan, in preparation). The laser pulse duration was 1.3 μ s and the dye used was coumarin 2, operating at ~ 450 nm. Then restricted ablations were made deep in one brain hemisphere without damaging overlying cells. A large variety of anatomical defects and their behaviour correlates were obtained during progressive refinement of the ablation parameters. By using this fate-correlation we were able reproducibly to eliminate the H- and V-cells in the lobula plate. The lesions were made in a postero-lateral area of the larval brain at a depth of 30–60 μ m from the brain surface, and with a diameter of ~ 20 μ m (corresponding to two to three cell diameters), 24–32 h after hatching. The larvae were reared individually and the eclosed adult flies were then tested for changes in behaviour as described below. After completion of the tests, the fly brains were examined histologically (Fig. 1).

In the 16 flies used here, all the large horizontally and vertically arranged profiles in the lobula plate were missing on the ablated side (except in three flies where residues of one to two vertical cells were detected), with the control side normal. The large H- and V-cells could easily be identified on the control side; therefore we conclude that these cells were missing on the ablated side. The lobula plate on the treated side was slightly

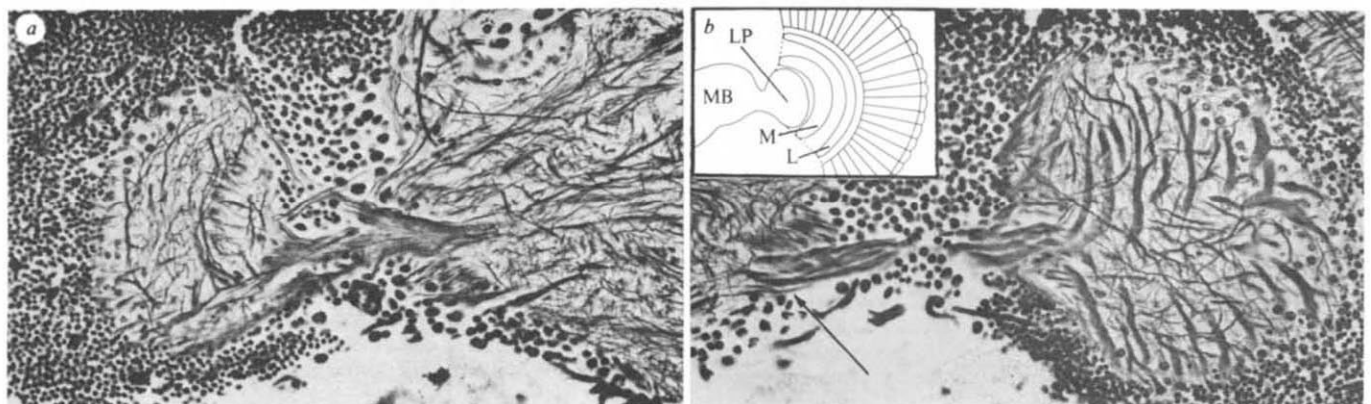


Fig. 1 Frontal (transverse) posterior-most section of lobula plates from the ablated (a) and normal (b) side of the same fly (reduced silver stain). Inset: half the brain is shown, with the optic lobes, lobula plate (LP), the medulla (M), the lamina (L), as well as part of the mid-brain (MB). The retina is peripheral to the lamina. In the micrograph only the large V-cells are shown and these are clearly distinguishable in the normal lobula plate, whereas only thin fibres remain on the treated side. The H-cells appear in deeper sections. Further evidence for the absence of the H- and V-cells is the lack of their large ipsilateral terminals in the posterior part of the mid-brain (compare with terminals on the normal side, marked by an arrow). The size of the lobula plate is slightly reduced on the treated side, although in the micrograph this difference is exaggerated, while the lobula plate on the treated side is obliquely oriented.

reduced in size. However, the remaining fibre pattern in the lobula plate appeared similar to that of the control side. The axonal projection pattern between the lobula plate and its adjacent neuropiles (medulla and lobula) on the ablated side also seemed normal. There was no detectable change in the structure of the retina and other optic neuropiles.

The optical stimulus used in the behaviour tests consisted of a long black vertical stripe which moved around the fly in the horizontal plane perpendicular to the fly's vertical axis. In flight, the fly was tethered to a torque-thrust transducer¹⁵ to measure its torque and thrust responses. In one test we measured the optomotor responses to clockwise or counter-clockwise transient movement of the stripe¹⁶ (Fig. 2). The average response of individual experimental flies did not deviate significantly from that of control flies. However, the response to the stripe moving in front of the ablated side was slightly (but not significantly) smaller and possibly more jerky than the response on the untreated side. The direction-sensitive and direction-insensitive optomotor components were calculated from this response¹¹, but revealed no significant differences between the ablated and control sides. In another test, the treated flies' ability visually to fixate a vertical stripe¹⁰ was measured and found to be more noisy but otherwise normal. Thus the mean position of the fixated stripe was in the direction of flight and the standard deviation of each fixation performed was on average not significantly larger than in untreated flies. The experimental fly's thrust response, measured simultaneously in these tests as a function of the horizontal movement of the stripe, also appeared normal in shape and strength. In addition, experiments were performed on 5 of the 16 flies, in which vertical gratings were moved horizontally clockwise and counter-clockwise in front of each eye separately. The results revealed a significant reduction in average turning response of 54% for both directions when stimulating the ablated side.

The reduction in the response to moving gratings on the ablated side and the normal response to a single moving stripe suggests that the H- and V-cells are mainly concerned with

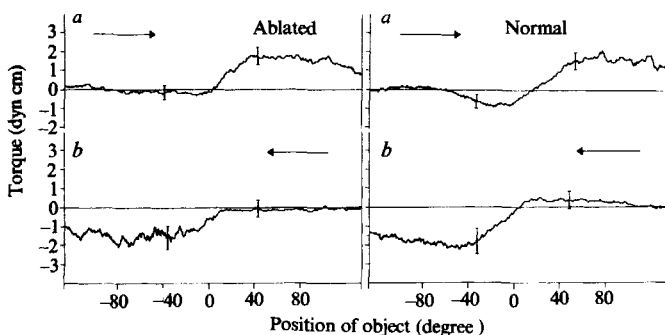


Fig. 2 Optomotor torque response of ablated and normal male flies to a moving, long, 5°-wide vertical black stripe, measured as the function of the position of the stripe around the fly. In flight, the fly was tethered to a torque-thrust transducer¹⁵, in the centre of a homogeneously illuminated background. The stripe was moved around the vertical axis of the fly, in front of the background, at a constant angular speed of 90° s⁻¹. It is moved clockwise from -135° to +135° (a) and after a pause of 8 s it was moved back from 135° to -135° (b); the direction of flight is defined as 0°. Torque was measured during the movement of the stripe and 1.5 s beforehand, to allow determination of zero torque line¹⁶. The tendency to turn to the right (torque to the right) was considered positive. Each fly was tested at least five times in each direction and this was averaged separately for each direction. The left-hand side shows the average response of three treated male flies, ablated on the left side. The response to progressive movement (from front to back) is similar on both sides and similar to the response of normal males right-hand side. However, the torque response on the ablated side (left) is slightly smaller and jerky. The response to regressive motion (from back to front) on the left side is normal, the small differences lying within the range of the standard deviation (vertical bars). Similar results have also been obtained with three males ablated on the right side and with 10 female flies.

wide-field motion detection whereas single-object motion detection is performed by other neurones. This is supported by electrophysiological studies²⁻⁶ demonstrating that the large H- and V-cells are responsible for wide-field motion detection.

From our results we conclude that these large horizontal and vertical neurones in the lobula plate are not crucial to visual orientation towards single objects but are involved in background motion detection and optomotor flight stabilization. Hence, we assume that at this level of information processing, the neurones in the lobula or the remaining neurones in the lobula plate are the ones essential for this orientation behaviour towards single objects in normal flies.

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Production of infectious Epstein-Barr virus in mouse lymphocytes

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Most studies of the course of primary infection by Epstein-Barr virus (EBV, associated with Burkitt's lymphoma and nasopharyngeal carcinoma^{1,2}) have been made with human and other primate B lymphocytes, the chief naturally susceptible host cell type³. *In vitro* infection of B lymphocytes results, however, in their transformation into permanent lymphoid cell types in which the lytic cycle is virtually suppressed. Infection of B lymphocytes is sharply restricted to those cells that carry surface receptors for immunoglobulin and the C3 component of complement. Infectibility may be entirely determined by the presence of EBV virus receptors, which is itself closely correlated with the presence of receptors for B cell-specific complement (C3d), but the establishment of the latent transformed state of host cells may well involve other internal characteristics of the differentiated B cell. We now report that, by means of the technique for implanting EBV receptors into the membranes of cells that lack them⁴, mouse lymphocytes can be infected with EBV resulting in a complete viral cycle, not in a state of latent transformation.

Mouse lymphocytes do not bind EBV. However, after receptor implantation⁴ they bound approximately half as much ³H-EBV as the receptor donor Raji cells (Fig. 1). The radioactivity persisted in the target cells for several hours in culture in a

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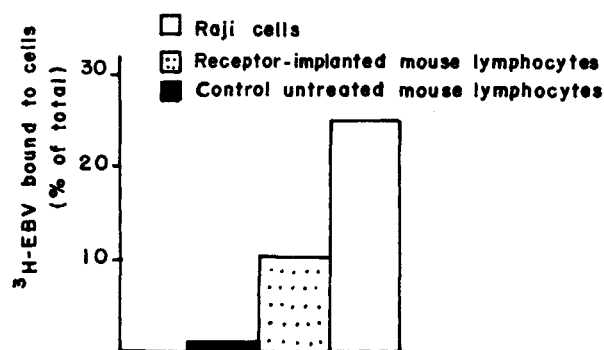


Fig. 1 Binding of ³H-thymidine-labelled EBV to receptor-implanted mouse thymocytes. EBV receptor-positive Raji cell membrane fragments were prepared and co-reconstituted with Sendai virus envelopes as described elsewhere⁴. The resulting hybrid Sendai virus envelope-EBV receptor vesicles (SRec-EBV) served for the implantation of EBV receptors into the membranes of the recipient cells. Thymocytes from 6-week-old CBA mice were suspended in a 160 mM NaCl, 20 mM Tricine-NaOH solution at pH 7.4 at a concentration of 2×10^7 cells ml⁻¹. SRec-EBV was added at 4°C. After 10 min, the vesicle-cell mixture was transferred to 37°C for 45 min followed by two washings. The cells were resuspended in RPMI-1640 medium supplemented with 10% fetal calf serum and antibiotics, and cultured in standard conditions (= receptor-implanted lymphocytes). Aliquots (10 µl) of ³H-EBV (2,000 c.p.m.) were added to 20×10^6 ml⁻¹ of the receptor-implanted or control (non-implanted) mouse lymphocytes, or to the same number of receptor-positive Raji cells. After 3 h in culture, the cells were washed, submitted to mild trypsinization (0.125% trypsin for 5 min at 20°C) and counted for the remaining cell-associated radioactivity.

trypsin-resistant form and therefore probably represented internalized virus.

To test the functional activity of EBV in the receptor-implanted mouse lymphocytes, the cells were exposed to P3HR-1 (P-EBV) or B95-8 (B-EBV) virus, cultured in standard conditions and stained periodically for EBV-determined nuclear, early and virus capsid antigens (EBNA, EA and VCA respectively). As shown in Table 1 and Fig. 2, all three antigens were demonstrable in the receptor-implanted cells but not in the unmanipulated, virus-exposed controls. This shows that mouse lymphocytes can be rendered EBV-susceptible by receptor implantation. However, their response differs from that of the human B-lymphocyte host cells in the following ways.

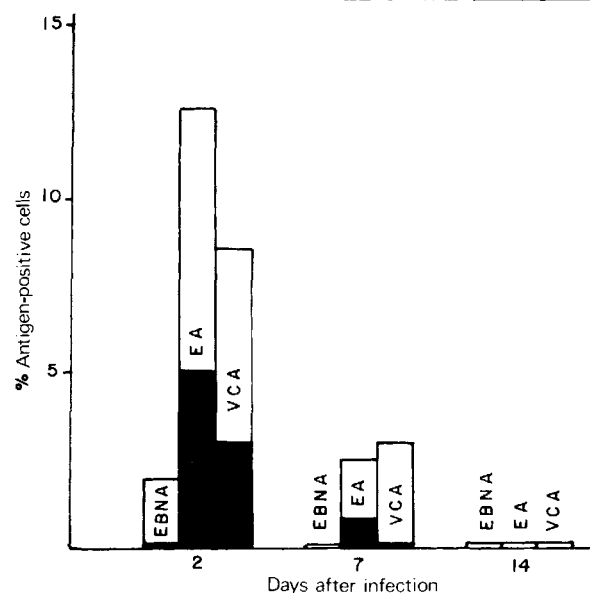


Fig. 2 Expression of EBV-determined antigens in mouse lymphocytes at different times after infection with two EBV substrains. The experimental system was as described in Table 1 legend. The cells were stained for EBV antigens at different times after infection. Open columns, P3HR-1 virus-infected cells; shaded columns, B95-8 virus-infected cells.

First, EBV-infectability of receptor-implanted murine lymphocytes was not restricted to the B-lymphocyte subpopulation. As shown in Table 1, mouse lymphocytes obtained from thymus, bone marrow or as a purified spleen cell fraction could be infected by the virus.

Second, in contrast to human lymphocytes, murine cells could be infected by both EBV substrains. It has been reported previously that the EBV substrain P3HR-1 is cytopathic for human B cells without inducing any detectable EBV antigen⁵. In murine lymphocytes, P-EBV induced all three antigens, although EBNA-positive cells were always present in lower numbers than EA- and VCA-positive cells (Table 1, Fig. 2). The B95-8 substrain of EBV⁶ induced only EBNA, never EA or VCA, in human B lymphocytes⁵, whereas in mouse lymphocytes it induced very little EBNA, with EA and VCA forming a significant fraction of the cell population (Table 1, Fig. 2).

Third, primary infection of human B lymphocytes by EBV is followed by the outgrowth of permanently transformed

Table 1 Induction of EBV-determined antigens and viral DNA replication in EBV receptor-implanted normal lymphocytes after EBV exposure

Cells	EBV substrain used for infection	% Positive cells			EBV DNA replication
		EBNA	EA	VCA	
Receptor-implanted Non-separated splenocytes	P-EBV	0.2; 1.0; 2.0	1.8; 6; 12.5	1.0; 5; 8.5	1.0; 4.7; NT
	P-EBV + Ara C	0.2	2.0	0	0
	B-EBV	0; 0.1; 0.1	1.0; 1.5; 5	0.8; 1; 3	0.3; NT; NT
Spleen B lymphocytes	P-EBV	0.2; 1.2	1.0; 7.0	0.3; 5.0	NT; NT
	P-EBV	0.1	1.5	1.0	NT
Bone marrow cells	B-EBV	0	0.3	0.2	NT
	P-EBV	0.2; 1.0; 2.2	1.5; 5.2; 7.5	0.9; 5.0; 5	1.0; 3.5; NT
Thymocytes	P-EBV + Ara C	0.2	1.0	0	0
	B-EBV	0	1.2	0.8	NT
Control					
Non-separated splenocytes; spleen B lymphocytes; bone marrow cells; thymocytes	P-EBV or B-EBV	0	0	0	0

Lymphocytes from 4–6-week old CBA or A/Sn mice were isolated by passing the tissues through nylon mesh. Erythrocytes were removed by hypotonic shock. Spleen B lymphocytes were obtained by lysing T lymphocytes with an anti- θ serum and rabbit complement; 95–97% of the remaining cells were surface immunoglobulin positive. Separated lymphocytes were implanted with Raji-derived EBV receptors, exposed to EBV and cultured for 2 days as described in Fig. 1 legend. Ara C was added to cells ($20 \mu\text{g ml}^{-1}$) 1 h after virus exposure. Cells were fixed in acetone/methanol (for antigen detection) or ethanol/acetic acid (DNA–DNA hybridization). EBNA was stained by the anti-complement immunofluorescence method¹¹; EA and VCA were detected by direct fluorescence⁹. In each specimen 1,000–5,000 cells were counted. EBV DNA–DNA hybridization was performed *in situ* with nick-translated ³H-thymidine-labelled EBV DNA as a probe, as described by Moar and Klein⁸. Exposure time was 7 days. 500 cells were counted in each specimen. Multiple figures in the same column represent separate experiments, performed with different receptor preparations and different EBV batches. Nine of 20 performed experiments are shown. P-EBV = EBV derived from P3HR-1 cells¹²; B-EBV = EBV derived from B95-8 cells⁸. DNA replication was detected by DNA–DNA hybridization *in situ*. NT, not tested.

(immortalized) lymphoblastoid cell lines that carry multiple copies of the viral genome and express EBNA³. No permanent immortalization of mouse lymphocytes has been achieved in 20 experiments performed so far. On the contrary, time course studies demonstrated a step decrease in the number of EBV antigen-positive cells, with their complete disappearance 2 weeks after infection (Fig. 2). Comparing these findings with the human lymphocyte system, it is tempting to assume that the non-transformability of murine lymphocytes by EBV may be due to the low inducibility of the nuclear antigen, particularly after infection with the transforming B95-8 substrain of the virus. However, the possibility of selecting transformed cells from the small population of EBNA-positive mouse lymphocytes cannot be ruled out.

The appearance of VCA-positive cells in EBV-infected mouse lymphocytes (Table 1, Fig. 2) suggests that the latter cells predominantly enter the full viral productive cycle.

During the lytic EBV cycle in cell lines, replicative viral DNA synthesis precedes the synthesis of late viral products, as occurs, for example, in the SV40 system. However, in SV40, the synthesis of large T antigens is a prerequisite for viral DNA replication⁷. As very little EBNA was induced in the artificially EBV-infected mouse lymphocytes, the question arose as to whether this might represent a bottleneck, preventing EBV DNA replication. This was examined by *in situ* EBV DNA-DNA hybridization performed according to Moar and Klein⁸. As seen in Table 1 and Fig. 3a-d viral DNA-replicating cells could be detected in 0.3-4.7% of the EBV-infected mouse cells. The EBV DNA-producing cells expressed 25-100 grains per cell (Fig. 3a-d) compared with 200-300 grains per cell in EBV producer P3HR-1 cells (Fig. 3e) and 2-3 grains per cell in controls. Thus the number of EBV genome equivalents per mouse-producer cell is probably lower than in P3HR-1 cells.

To test whether the lytically infected mouse cells released infectious virus, media were collected from EBV-infected mouse lymphocytes and tested for infectivity against Raji and Lukes cells. In the EBV genome-carrying Raji cells, the virus was tested for EA induction by superinfection, similar to studies with the P3HR-1 substrain⁹, and the Lukes cells, an EBV-negative B-lymphoma line (E. Kieff, personal communication), were tested for EBNA induction. As shown in Table 2, both

Table 2 Infectivity of EBV released by receptor-implanted, P-EBV-infected mouse lymphocytes (M-EBV), judged by the induction of EBV-determined antigens

Supernatant source	Target cells	Antigen-positive cells (%) EBNA EA
Receptor-implanted mouse lymphocytes infected with P-EBV	Raji Lukes	3.5; 5.0; 3.5, 1.5; 4.5; 8.1
Receptor-implanted mouse lymphocytes infected with P-EBV in the presence of Ara C	Raji Lukes	0 0
Mouse lymphocytes mock-infected with P-EBV (no receptor implantation)	Raji Lukes	0 0

Receptor-implanted mouse lymphocytes were infected with P-EBV in the presence or absence of Ara C ($20 \mu\text{g ml}^{-1}$). Unadsorbed virus was removed 12-24 h after infection by washing the cells twice and resuspending them in a fresh medium. Mock-infection was performed by adding EBV to untreated lymphocytes, which were then processed as the receptor-implanted cells. Culture media were collected 7-14 days after infection or mock-infection. All culture media collected from infected cells containing at least 3% VCA-positive cells were combined, filtered ($0.45 \mu\text{m}$, Millipore) and centrifuged for 2 h at $31,000g$. The resulting pellet was resuspended in 2% of the original volume and designated as M-EBV. Culture media of Ara C-inhibited and mock-infected cells were treated as above (no pellets were obtained after centrifugation). Target cells were exposed to these preparations, fixed after 2 days and stained for antigens as described. Three separate experiments with different M-EBV preparations are shown.

tests gave positive results. Media collected after mock-infection of untreated cells or after infection of receptor-implanted lymphocytes in the presence of cytosine arabinoside (Ara C) showed no infectivity (Table 2), thus demonstrating that the observed exogenous infectivity of the medium was not due to the possible release of an input or adsorbed virus.

We conclude that EBV-infected mouse lymphocytes are capable of releasing biologically active EBV, referred to as M-EBV. Several attempts to demonstrate the presence of biologically active M-EBV after infection of the cells with the EBV B95-8 substrain have been unsuccessful, perhaps due to a low level of virus-induced VCA and viral DNA replication (Table 1; compare also Fig. 3a and d).

We have thus found that a complete EBV-lytic cycle can be induced by primary infection of normal mouse lymphocytes. The fact that a cycle proceeds with little detectable EBNA challenges the notion that EBNA synthesis, like papova virus T antigens, is an obligatory early step in the lytic cycle¹⁰. Conceivably, there may be two different, mutually exclusive pathways of EBV expression: the EBNA-positive transformation pathway in the human B lymphocytes and the lytic pathway in other primarily infected cells that may not involve EBNA synthesis.

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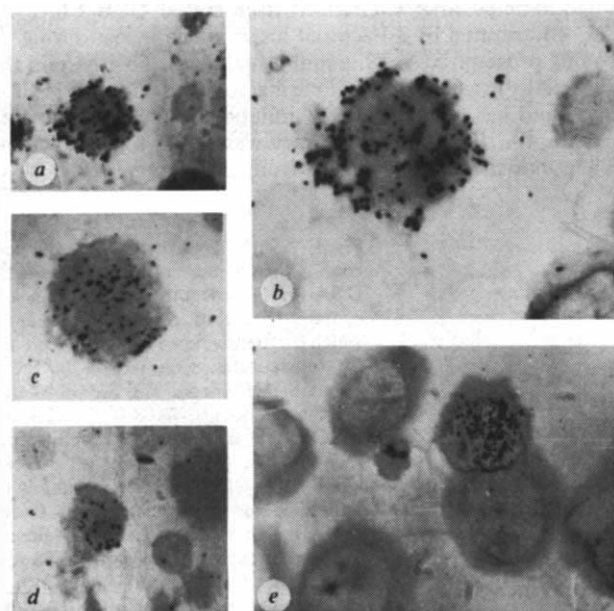


Fig. 3 Detection of EBV DNA replication in mouse lymphocytes by cytohybridization. Mouse lymphocytes were implanted with EBV receptors, exposed to EBV and subjected to *in situ* ³H-EBV DNA-DNA hybridization 2 days after infection, as described in Table 1 legend. a-c, P3HR-1 virus-infected cells; d, B-95-8 virus-infected cells; e, control P-3HR-1 cells. a, c, d, e, $\times 300$; b, $\times 650$.

Chi, a promoter of generalized recombination in λ phage, is present in immunoglobulin genes

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Generalized recombination is a phenomenon dependent on recognition of homologous DNA and may be responsible for generating a mouse $\gamma 2b$ - $\gamma 2a$ hybrid immunoglobulin heavy chain¹. The genetic element, Chi, has been shown to enhance generalized recombination in prokaryotes²⁻¹⁰. Sequence comparison predicted Chi to be present in the C_H3 domain of the mouse $\gamma 2b$ gene, and using plaque size in the appropriate selective conditions as a measure of Chi activity^{2,3}, a clone containing the $\gamma 2b$ gene¹¹ was shown to confer Chi activity on the λ vector which carried it. Thus Chi, if functional in eukaryotic systems, could have promoted the recombinational event which produced the $\gamma 2b$ - $\gamma 2a$ hybrid immunoglobulin. We report here that the frequency of Chi in the mouse genome is similar to that in other eukaryotic unique sequences but lower than in immunoglobulin genes. The positions at which Chi occurs in immunoglobulin genes and the fact that the $\gamma 2b$ - $\gamma 2a$ hybrid variants isolated from mouse myeloma cell line MPC11 may represent an aberrant class switch suggest that homologous recombination may be broadly applicable to the heavy-chain class switch.

An immunoglobulin heavy chain gene is encoded by several discrete genetic elements: variable (V) region, D segment, joining (J) segment and constant (C) region. During differentiation, these genes apparently undergo two kinds of DNA rearrangement. The initial rearrangement, V-D-J joining, juxtaposes a V region to one of five J segments located 5' to the μ constant-region gene in the germ line¹²⁻¹⁴. Secondary rearrangements involve the transposition of the intact V-D-J region from the μ constant-region gene to one of seven other constant-region genes, accompanied by deletion of the intervening genetic material¹⁵⁻¹⁸. These secondary rearrangements are thought to be the basis of the heavy-chain class switches observed on antigenic stimulation.

Similar changes in heavy-chain class expression occur in mouse myeloma cell lines¹⁹⁻²³. We have shown that mutagenesis of the IgG2b producing mouse myeloma cell line, MPC11, has yielded various variant cells producing constant regions bearing previously unexpressed $\gamma 2a$ serological and structural

markers^{19,24}. Although some variants may synthesize complete $\gamma 2a$ constant regions, others are actually $\gamma 2b$ - $\gamma 2a$ hybrid heavy chains¹. Primary structural analysis of ICR11.19.3, a $\gamma 2b$ - $\gamma 2a$ hybrid, has localized the potential cross-over point to a sequence shared by the $\gamma 2b$ and $\gamma 2a$ genes^{1,25-27}. This suggests that a mechanism dependent on this homology, such as generalized recombination, may have been used to generate ICR11.19.3.

Special sites have been shown to influence recombination rates in organisms such as *Drosophila* and fungi; however the identity of these sequences has not been determined²⁸⁻³². In contrast, sites in bacteriophage λ which promote generalized recombination have been mapped genetically and analysed²⁻⁵. DNA sequence analysis of these Chi sites has localized Chi

activity to the nucleotide sequence 5' GCTGGTGG 3' (refs CGACCACC³) (refs 6-10). The presence of the Chi sequence in the third domain of the mouse $\gamma 2b$ constant-region gene suggested to us that, by analogy to its activity in the λ -*Escherichia coli* system, Chi may have stimulated the putative generalized recombination event which gave rise to the $\gamma 2b$ - $\gamma 2a$ hybrid ICR11.19.3.

In λ phage, Chi sites seem to promote generalized recombination in an asymmetric fashion, acting over distances of 5,000-10,000 base pairs^{4,5}. These sites do not occur in wild-type λ but can be selected for when the phage's own recombination system, Red, has been eliminated and its Gam protein inactivated³³. In these circumstances, phage cannot switch from Cairn's θ -type replication to rolling-circle replication which produces concatemers, a requirement for efficient encapsidation³³. Thus the phage is forced to use the RecBC pathway, *E. coli*'s major recombination system, which directs recombination of monomeric circles to form dimers; these structures can be used instead of concatemers as substrates for packaging into phage heads³³. Although this system is used inefficiently by Red⁻Gam⁻phage whose phenotype is the production of small plaques, a mutation generating a Chi⁺ sequence in phage λ will increase the frequency with which dimers are formed, increasing the burst size and thus the plaque size^{2,3,34,35}.

To determine whether the Chi sequence in the $\gamma 2b$ C_H3 domain could facilitate generalized recombination in λ phage even though residing in the DNA environment of an immunoglobulin constant-region gene, SL59, a $\gamma 2b$ genomic clone in the λ vector Charon 4A (ref. 11), was tested for Chi phenotype and found to be positive. Charon 4A itself is Red⁻Gam⁻Chi⁺, with Chi⁺ contributed by a bacterial insert which is lost during the cloning procedure (A. K., unpublished data). To ascertain that Chi activity in a Charon 4A clone is associated only with inserted DNA and has not arisen as a mutation in the phage arms, a Charon 4A, *EcoRI* mouse library was tested for Chi activity. When individual recombinants from this library were assayed

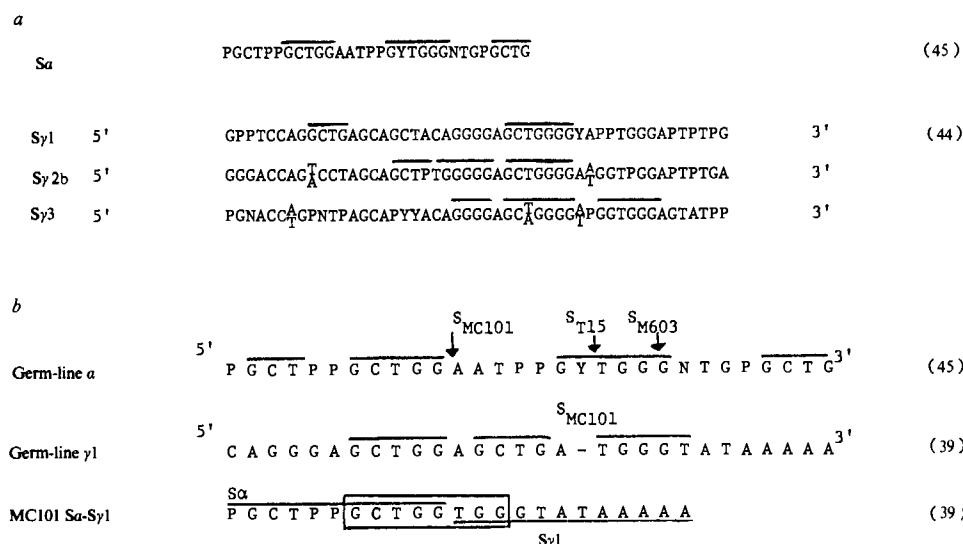


Fig. 1 Switch DNA consensus sequences contain the elements of Chi. a, S_μ and S_α DNA are homologous to each other and contain blocks of repetitive sequences which frequently repeat the first 7 out of 8 Chi nucleotides. Comparison of the γ family S-DNA to S_μ and S_α shows that the short sequences AGCT, TGGG and AGCTGGG occur at the recombination sites and are shared among all S-DNA types⁴⁶. Therefore Chi can potentially be formed in any heavy-chain class switch just as it is in MC101. b, A Chi sequence was formed at the juxtaposition of S_α and $S_{\gamma 1}$ in the myeloma MC101. Honjo *et al.*⁴⁶ have proposed that MC101 results from a series of unequal crossing-over events between sister chromatids, a mechanism that could also account for 'revertants' in class switching²³ and which could be facilitated by Chi. References from which the sequences were taken are given in parentheses.

for Chi phenotype by plaque size, both large (Chi⁺) and small (Chi⁻) plaques were observed. When present, Chi must thus be associated only with inserted DNA. Therefore, Chi⁺ activity in the genomic clone, SL59, must be associated with the insert, which contains the $\gamma 2b$ gene.

Many λ vectors, including Charon 4A, do not contain Chi in their arms (F. Blattner, personal communication). As Chi confers a selective growth advantage and can arise by a single base mutation, Chi sequences observed in unique sequence DNA might be cloning artefacts. However, the $\gamma 2b$ constant-region gene has been independently sequenced four times and shows a Chi at the same position in each case^{25,26,36,37}. Importantly, one of these sequences was of a cDNA clone which had not been passed through a λ vector and thus presumably not brought under selective pressure for Chi²⁶. It is unlikely that four independent base substitutions would have occurred at the same site to generate a Chi, while other positions were equally available. Thus, at least in the case of the $\gamma 2b$ sequence, the Chi octamer apparently did not arise as a cloning artefact.

Published immunoglobulin DNA sequences containing the Chi sequence include: (1) the 3'-flanking region of mouse κ light chain³⁸; (2) the heavy-chain switch DNA, located 5' to the $\gamma 1$ constant-region gene of myeloma MC101 and implicated in intrachromosomal constant-region transposition during the heavy-chain class switch³⁹; and (3) the heavy-chain variable regions of murine myelomas S107, M603, MOPC21 and the human hybridoma Sp1/HL (refs 14, 14, 40 and 41, respectively). A. Taylor and G. Smith have observed that six immunoglobulin Chi sequences occurred within 18 kilobases (kb) of DNA while only four Chi octamers were present in 70 kb

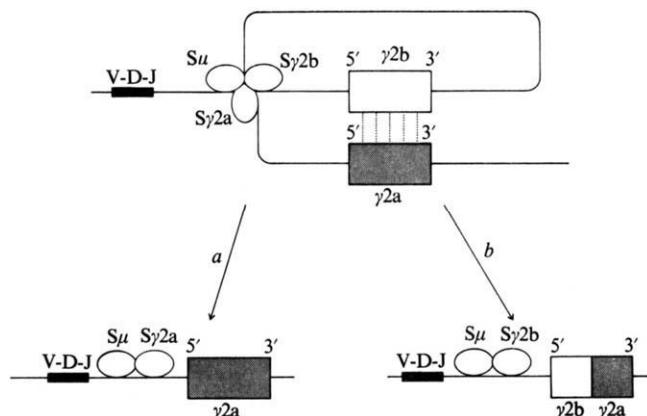


Fig. 2 Proposed intermediate in heavy-chain class switch. The example is based on the MPC11 variant ICR11.19.3 and shows that $S\mu$ and $S\gamma 2b$ regions are contiguous and where the $S\gamma 2a$ switch region DNA is in close proximity. The whole structure is presumed to be stabilized by homologies in the various switching regions and particularly by homologies amongst the constant-region genes. Chi may promote but is not necessary for the generation of this intermediate. *a*, Putative switching enzymes are induced and recognize, bind to and cause recombination in switch DNA resulting in a new variable region-constant region combination. *b*, Hypothetical recombination enzymes are activated which recognize and act on DNA homologies to promote generalized recombination between the constant regions, resulting in a hybrid constant region.

Table 1 Frequency of Chi in mouse genomic DNA

DNA insert size (kb)	Amount of mouse DNA screened* (kb)	Frequency of Chi per fragment†	Frequency of Chi‡ (per kb)
Minimum insert (8.2)	393.6	0.58	14.1
Maximum insert (15.3)	734.4	0.58	26.4
Average insert (10.3 ± 2)	492.5	0.58	17.8

A random distribution of Chi or any sequence of 8 nucleotides, when present on either strand of DNA, can be expected to occur once in 33 kb ($4^8 \div 2$), assuming a 50% G+C content. The mouse has a 42% G+C content⁴⁰, so the expected frequency of Chi in mouse is once in 70 kb. This expected Chi frequency for mouse, as calculated statistically, is lower than the Chi density for the mouse genome (once in 17 kb) and very different from the Chi frequency in immunoglobulin genes (every 3 kb). A similar observation was made in *E. coli* where Chi was found once in 5–10 kb, a frequency higher than that predicted⁴⁸. The major difference between mouse and *E. coli* DNA is that in bacteria most of the DNA is used for coding, whereas in mouse most is non-coding. The inability statistically to predict Chi frequency may be because nucleotide usage is non-random or because the Chi octamer is selected for in both prokaryotes and eukaryotes. A fresh genomic mouse library was prepared from a variant of MPC11, ICR11.19.3, by partial *MboI* digestion and subsequent ligation into the Red-Gam⁻ λ vector Charon 28 (ref. 49), prepared with *Bam*HI. A portion of the unamplified library was modified on *E. coli* C600 *r*-*m*⁺ (LE392) and 48 plaques were chosen randomly. Each was analysed by propagation on 594 P2 and growth on BBL-Trypticase⁵⁰, which discriminates between the plaque sizes produced by Chi⁺ and Chi⁻ λ ². In this random sample, 12 of the plaques were Chi⁺. Chi⁺ activity is found only when the sequence is oriented in the appropriate direction with respect to λ phage DNA arms⁵¹. Therefore, a portion of the plaques scored as Chi⁻ probably contained the Chi sequence positioned in the inactive orientation. In addition, if a single restriction fragment carries more than one Chi, it will only be scored as one in this assay. Assuming that the number of Chi sites per fragment follows a Poisson distribution, the frequency of Chi⁻ plaques, 36/48, is the frequency that Chi is not present on a DNA fragment in the active orientation. Using the statistical argument $P_0 + e^{-m} = 36/48$; $-\ln P_0 = m$, the mean number of Chi sites per fragment in the active orientations. As Chi can occur with equal frequency in the inactive orientation, there are 0.58 expected Chi sites per fragment cloned. The average insert size was determined by restriction analysis of 10 ICR11.19.3 library clones. Phage DNA was obtained by a rapid extraction method (B. Wydro, personal communication). One μ g of DNA was taken for each digestion with *Bgl*II or *Eco*RI. Digests were run on a 0.8% agarose gel and restriction fragments sized.

* Insert size $\times$ number of clones examined.

† $P_0 = e^{-m}$ where $-\ln P_0 = m$, the mean number of Chi sites per fragment in the active orientation.

‡ $=(\text{Frequency of Chi per fragment}) \times (\text{size of fragment in kb})$.

of other eukaryotic DNA sequences (personal communication).

It was therefore interesting to determine experimentally whether the mouse genome reflects a Chi distribution typical of unique eukaryotic DNA, or some heightened frequency as found in immunoglobulin DNA sequences. To determine this, a portion of the unamplified library of the variant ICR11.19.3 was plated and 48 plaques chosen at random. Each was analysed in conditions which discriminate between Chi phenotypes (see Table 1). From this sample, 12 of the 48 plaques were Chi⁺; thus the frequency of Chi in mouse DNA could be calculated to be 1/17 kb (Table 1), based on a Poisson distribution and an average insert size of 10.3 ± 2 kb. Therefore, the frequency of Chi in immunoglobulin DNA—once in 3 kb—is higher than that in the mouse genome and other unique eukaryotic DNA sequences. The functional significance of this increased frequency is unknown, but is speculated on below. The functional assay for detecting Chi relies on the ability of the Chi sequence to enhance recombination in λ phage when propagated on the appropriate *E. coli* host. As a similar assay for Chi cannot yet be carried out in a eukaryotic system, we can only speculate as to the nature of this sequence's function, if any, in higher organisms.

The positions at which Chi resides in immunoglobulin DNA may help elucidate this problem. Chi is present within the four V_HIII-subgroup V regions of S107, M603, MOPC21 and Sp1/HL at nucleotides 21–28 corresponding to N-terminal framework amino acids 3–6^{14,40,41}. By generating the putative DNA sequence corresponding to amino acids N3 to N6, 73% of mouse, 70% of human and all seven rat V_HIII-subgroup V regions are potentially Chi⁺ (ref. 42). The most common amino acid sequence (or protein consensus sequence) for all species is predicted to be Chi⁺, by the same reasoning⁴². Chi is similarly predicted to be absent in human and mouse subgroups V_HI and V_HII and mouse subgroups V_HIV and V_HV at the position discussed above. Using antisera specifically directed against human V_H subgroups, it has been found that half the V regions expressed belong to the V_HIII subgroup⁴³. Chi may have functioned preferentially to expand the genes of the V_HIII subgroup by unequal crossing-over or to promote the subgroup's gene expression.

Although, of the heavy-chain constant region genes, only the

$\gamma 2b$ gene has been shown to contain Chi, the formation of Chi in the rearrangement of switch (S) DNA leading to expression of the $\gamma 1$ heavy chain in the MC101 myeloma suggests a more general role in the heavy-chain class switch. Switch DNA is located 5' to each of the heavy-chain constant regions and is used to facilitate the intrachromosomal transposition or sister chromatid exchange of the V region expressed with $C\mu$ in the primary response to the constant region(s) subsequently to be expressed^{13,39,44,45}. It has been reported that the switch-region DNAs of μ and α are notably homologous to each other and contain blocks of repetitive sequences⁴⁵. These sequences, which frequently repeat the first seven of eight Chi nucleotides, are contained within the consensus sequence for the S-DNA recombination site and are capable of forming the complete Chi octamer on switch recombination. Although the γ family of S-DNA is unlike that of μ and α , the short sequences AGCT, TGGG and AGCTGGGG occur at recombination sites and are shared between $S\mu 1$, $S\gamma 1$, $S\gamma 2b$, $S\gamma 3$ and $S\alpha$ ⁴⁶. It is therefore possible to generate a Chi sequence in almost every class switch (Fig. 1). As Chi stimulates homologous recombination, it may be that, once formed in the S-DNA, Chi can facilitate subsequent heavy-chain class switches.

As noted previously, Chi in the $\gamma 2b$ heavy chain may have been involved in the generation of a hybrid $\gamma 2b$ - $\gamma 2a$ variant from the MPC11 $\gamma 2b$ myeloma cell line. This variant is one of a group of four $\gamma 2b$ - $\gamma 2a$ hybrid chains isolated, suggesting that the molecular pathway leading to such a hybrid is not infrequently used. The hybrids may have been the molecular consequence of an aberrant heavy-chain class switch, involving the recognition of base homologies within the constant-region gene in addition to the constant-region switching sites. The looped structure shown in Fig. 2 illustrates how an intrachromosomal recombination intermediate may be stabilized by homology shared by the constant-region genes and S-DNA. Resolution of the depicted intermediate recombinant structure could thus lead to switching or to hybrid heavy chains.

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Glucocorticoids and benzo(a)pyrene have opposing effects on EGF receptor binding

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The potent tumour promoter 12-*O*-tetradecanoyl-phorbol-13-acetate (TPA), and related phorbol ester tumour promoters, act by binding to specific high affinity receptors on the cell surface and this leads to changes in cell membrane structure and function, as well as highly pleiotropic effects on gene expression¹⁻³. The ability of the phorbol ester tumour promoters⁴⁻⁷, and of a recently discovered tumour promoter, teleocidin⁸, to induce inhibition of the binding of epidermal growth factor (EGF) to its cell-surface receptors provides a convenient marker for the membrane effects of these tumour promoters. The fact that repeated applications of benzo(a)pyrene (BP) alone to mouse skin will elicit tumours suggests that it can function as both an initiator and a promoter^{9,10}. Thus, in addition to its genotoxic effects, BP might induce membrane effects that resemble those seen with tumour promoters. Indeed, we have found that BP and certain other polycyclic aromatic hydrocarbons (PAHs) are potent inhibitors of EGF-receptor binding in the mouse embryo fibroblast cell line C3H10T $\frac{1}{2}$ (ref. 11). As glucocorticoids can inhibit PAH carcinogenesis^{12,13} and specifically inhibit tumour promotion on mouse skin^{14,15}, we have also examined their effects on EGF receptors. We report here that glucocorticoids alone induce an increase in EGF-receptor binding in C3H10T $\frac{1}{2}$ cells and that pretreatment of the cells with a glucocorticoid opposes the inhibitory effects of both TPA and BP. Thus glucocorticoids may inhibit the carcinogenic process by inducing membrane effects that are reciprocal to those induced by carcinogens and tumour promoters.

Figure 1 indicates that exposure of C3H10T $\frac{1}{2}$ cells to a non-toxic dose of BP (1×10^{-6} M) led to marked inhibition of EGF-receptor binding, inhibition being maximal at ~24 h. This is in contrast to the inhibition obtained with TPA or teleocidin,

which is maximal within a few minutes⁴⁻⁸. Therefore, we assume that although TPA can exert direct effects at the level of cell membranes¹, BP inhibition of EGF binding is mediated through indirect effects dependent on cytoplasmic and/or nuclear events. Additional studies, described elsewhere¹¹, indicate that several PAHs were able to inhibit EGF-receptor binding on C3H10T $\frac{1}{2}$ cells with the same time course and in the same range of concentrations ($1-4 \times 10^{-6}$ M) as those observed for BP. The order of potency for the compounds studied was: dimethylbenz(a)anthracene > BP > benza(a)anthracene = 3-methylcholanthrene > β -naphthoflavone. These findings have led us to propose¹¹ that BP and certain other PAHs can act indirectly, via the Ah receptor system^{16,17}, to induce membrane changes which mimic those produced by the phorbol ester tumour promoters and that this might explain their ability to act as complete carcinogens.

In contrast to the effects of certain PAHs, we found that exposure of C3H10T $\frac{1}{2}$ cells to the potent glucocorticoid flucinolone acetonide (FA) led to significant stimulation of EGF-receptor binding (Fig. 1). The stimulation was apparent within ~1 h and reached a maximum by 24 h. Maximal stimulation of EGF binding by FA was ~1.9 times that of control cultures. We tested several other glucocorticoids at 10^{-6} – 10^{-9} M and found that they also stimulated EGF-receptor binding with a time course similar to that shown in Fig. 1. The minimal concentrations of each of the glucocorticoids required to produce a 1.75-fold stimulation of EGF-receptor binding were: FA, 1×10^{-9} M; dexamethasone, 1×10^{-8} M; β -methasone, 2×10^{-8} M; triamcinolone acetonide, 1×10^{-7} M; and hydrocortisone, 2×10^{-7} M. This order of potency agrees with previous data on the relative potencies of these compounds

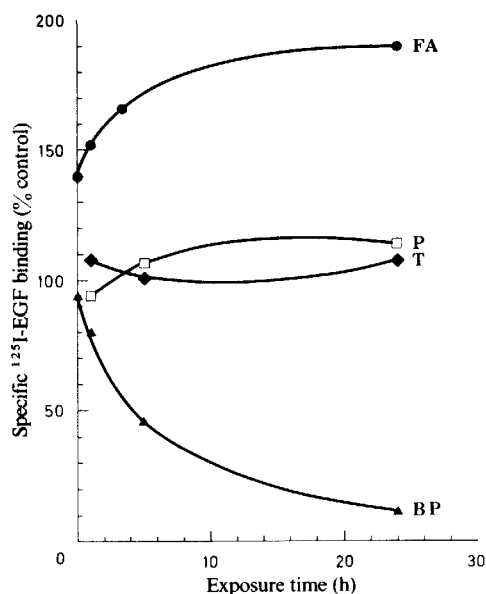


Fig. 1 Effects of incubation of C3H10T $\frac{1}{2}$ cells with 2×10^{-7} M FA, 1×10^{-6} M BP, 1×10^{-6} M progesterone (P), or 4×10^{-6} M testosterone (T), on specific binding of 125 I-EGF. Confluent cultures (10^6 cells per 5-cm diameter dish) were exposed to the test compounds for various periods of time (0–24 h) at 37°C in complete growth medium, before the EGF-binding assay. Assays for specific binding of EGF were done on intact cells, by incubating the cell monolayers with 125 I-EGF (15 pM) at 37°C for 50 min, as described elsewhere⁷. For the 'zero' time points the test compound was added directly to the binding assay. Assays of total binding of 125 I-EGF were always run in parallel with assays of nonspecific binding of 125 I-EGF (that obtained in the presence of a vast excess of unlabelled EGF (16 nM)). Nonspecific binding represented less than 15% of the total 125 I-EGF bound. This value was subtracted from the total to obtain the specific binding. Data are expressed as a percentage of the control, that is, specific 125 I-EGF binding in the absence of the test compounds, which was ~3,000 c.p.m. per 10^6 cells. Each point represents the mean of duplicate culture plates. Duplicate samples varied by <10%.

with respect to anti-inflammatory activity¹⁸, ability to inhibit TPA-induced promotion on mouse skin^{14,15} and ability to inhibit methylcholanthrene-induced subcutaneous sarcomas in mice^{12,13}. The stimulatory effect is specific to the glucocorticoid steroids as, when tested at concentrations as high as 10^{-6} M, progesterone, testosterone, cholesterol, 17 β -oestradiol and pregnenolone-16 α -carbonitrile did not significantly affect EGF-receptor binding in C3H10T $\frac{1}{2}$ cells (Fig. 1 and additional data, not shown).

In view of the ability of glucocorticoids to inhibit carcinogenesis¹²⁻¹⁵, it was of interest to determine whether FA could antagonize the inhibitory effect of BP or TPA on EGF-receptor binding. C3H10T $\frac{1}{2}$ cells were pretreated with solvent or FA for

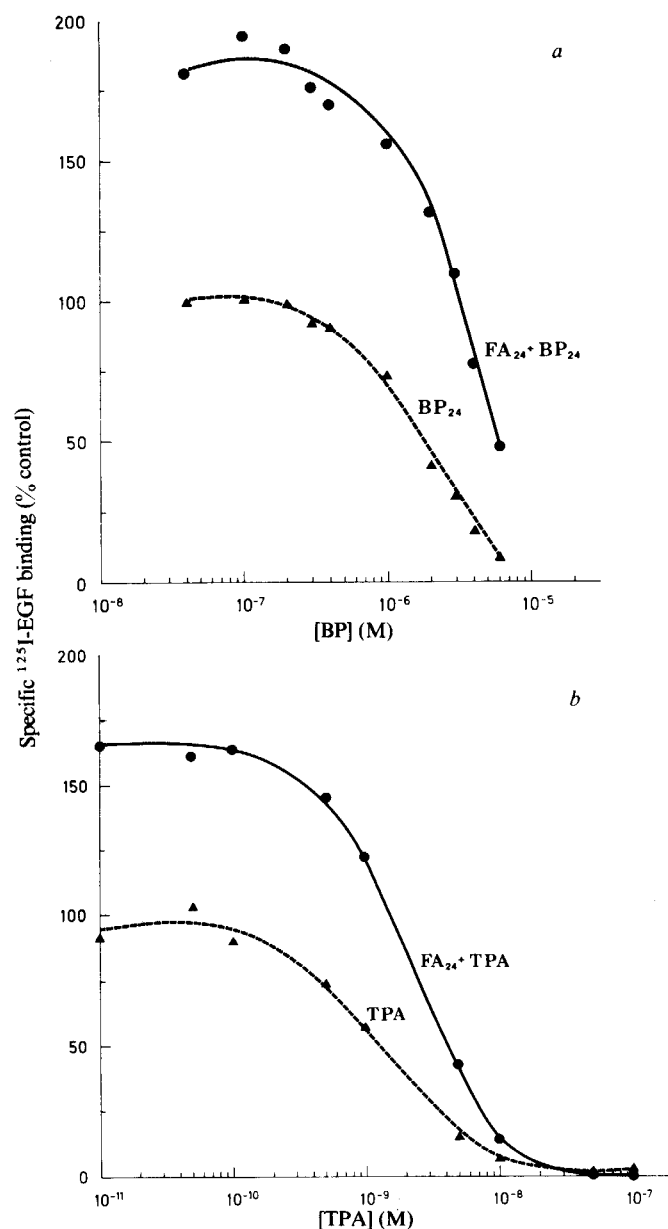


Fig. 2 Effect of pretreatment of cells with FA on BP (a) or TPA (b) inhibition of EGF binding to C3H10T $\frac{1}{2}$ cells. Confluent cultures were incubated in the absence or presence of 2×10^{-7} M FA for 24 h (FA₂₄). In a, all the cultures were exposed for an additional 24 h to the indicated concentration of BP (BP₂₄) in the continued absence or presence of FA. In b, the cultures were washed and assayed for binding of 125 I-EGF in the presence of the indicated concentrations of TPA. The 125 I-EGF-binding assays were performed as described in Fig. 1 legend. Data are expressed as % of control, that is, specific EGF binding obtained in cultures not exposed to FA, BP or TPA. Each point represents the mean of triplicate dishes, which differed by less than 10%.

24 h, then exposed for an additional 24 h to various concentrations of BP before being assayed for EGF-receptor binding (Fig. 2a). In this experiment, in the absence of FA pretreatment, EGF binding was inhibited by ~50% by 2×10^{-6} M BP. However, cells pretreated with FA and then exposed to 2×10^{-6} M BP showed a level of EGF binding greater than that of control cultures (that is, cells not exposed to FA or BP). High concentrations of BP ($> 4 \times 10^{-6}$ M BP) were able to reverse the protective effect of FA. When cells were pretreated with FA the concentration of BP required to produce 50% inhibition of EGF binding (with respect to control cultures) was 3–4-fold greater than that required in the absence of FA.

Figure 2b indicates that FA pretreatment also protected cells from TPA inhibition of EGF binding. In this experiment cells were pretreated with FA or solvent for 24 h and then assayed for EGF binding in the presence of various concentrations of TPA, because previous studies had established that TPA inhibition of EGF binding occurs within a few minutes of exposing the cells to the tumour promoter^{4–8}. In the absence of pretreatment with FA, ~50% inhibition of EGF binding occurred at 10^{-9} M TPA. In the case of the FA-pretreated cells, EGF binding in the presence of 10^{-9} M TPA was greater than that in control cultures. As with BP, high concentrations of TPA were able eventually to reduce EGF binding to a level below that of the control cultures in FA-pretreated cells. Compared with control cultures, a ~fourfold higher concentration of TPA was required to produce 50% inhibition of EGF binding in the FA-pretreated cells.

These results indicate that FA is able to protect cells from the inhibitory effects of BP or TPA on EGF-receptor binding. These results may be of interest in terms of carcinogenesis because, as discussed above, PAHs can act as complete carcinogens and glucocorticoids can inhibit both PAH-induced carcinogenesis and TPA-induced tumour promotion. Note that glucocorticoids and tumour promoters also have reciprocal effects on various other biological phenomena. On mouse skin, TPA induces epidermal hyperplasia and tumour promotion^{9,10}; in cell culture it induces plasminogen activator, decreases fibroblast adhesion, stimulates growth in agar, induces arachidonic acid release, and enhances deoxyglycose uptake (for review see ref. 1). Glucocorticoids have been demonstrated to have the opposite effects on each of these phenomena in normal cells^{14,15,19–21}. In general, TPA induces changes in normal cells that mimic those seen in transformed cells¹, whereas glucocorticoids tend to induce phenotypic reversion of transformed cells^{20,21}. In view of the present findings with respect to EGF receptors, we suggest that many of the antagonizing effects of tumour promoters and glucocorticoids reflect reciprocal effects of these compounds at the level of cell-surface membranes. Occupancy of the cytosolic glucocorticoid receptors presumably leads to alterations in gene expression that include effects on cell-surface membrane structure and function²¹. Baker and Cunningham²² have previously noted that human fibroblasts exposed to glucocorticoids have an increased affinity for EGF, a finding consistent with our results for C3H10T $\frac{1}{2}$ cells. Although further studies are required to elucidate the underlying mechanism, our results emphasize the important role that cell surface changes may have in both carcinogenesis and anti-carcinogenesis.

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Generation of long mRNA for membrane immunoglobulin $\gamma 2a$ chains by differential splicing

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Immunoglobulin may be secreted by a lymphocyte or bound to the cell membrane as an antigen receptor¹. The most common classes of membrane immunoglobulin (mIg) are IgM and IgD, but some B lymphocytes, thought to be involved in immunological memory^{2,3}, bear other classes such as mIgG and mIgA⁴. In mice, the μ_m heavy chain of mIgM bears a hydrophobic C-terminus whereas in the secreted (μ_s) chain it is hydrophilic. There are distinct μ_s and μ_m mRNAs with different 3' termini, which are produced by alternative splices of transcripts of the active rearranged μ gene^{5–7}. There also seem to be alternative C-termini and distinct mRNAs for δ_s and δ_m chains^{8–11}. The murine B-lymphoma cell line 2PK-3 (ref. 12) produces both membrane and secreted IgG2a¹³. The $\gamma 2a_m$ polypeptide is larger and more acidic than the $\gamma 2a_s$ chain^{13,14}, but nothing is known about its C-terminus or about the mRNA and gene regions which encode it. We report here the identification of a 3.9-kilobase (kb) $\gamma 2a$ RNA species in 2PK-3 which is spliced at the 3' end in a different way to the 1.9-kb $\gamma 2a_s$ mRNA. We suggest that this very long RNA is the mRNA for membrane $\gamma 2a$ chains.

To identify $\gamma 2a$ RNA species in 2PK-3, we fractionated total poly(A)-containing RNA by size using electrophoresis, blotted it on to diazobenzyloxymethyl (DBM) paper¹⁵ and hybridized it to a $\gamma 2a$ probe (probes *b* or *d* in Fig. 1A). As shown in Fig. 2 (lanes *a*, *i*), two species of $\gamma 2a$ RNA were detected, approximately 1.9 and 3.9 kb long. In the RNA preparations shown in Fig. 2, these were present at an average of 50 and 10 molecules per cell respectively, as determined by densitometry of autoradiographs exposed for various times (M173 was assumed to contain ~2,000 molecules of $\gamma 2a$ mRNA per cell; S. Cory, personal communication). However, for unknown reasons, the level of the 3.9-kb species varied in different cell preparations to as low as an average of one molecule per cell, even in cloned sublines. As plasmacytomas producing only secreted IgG2a, such as MOPC173 (Fig. 2, lane *b*), contain only the 1.9-kb species, in 2PK-3 this RNA is presumably the $\gamma 2a_s$ mRNA. The molecular weight of the $\gamma 2a_m$ polypeptide is 10,000 (~90 amino acids) greater than that of the $\gamma 2a_s$ polypeptide¹³; the $\gamma 2a_m$ mRNA must therefore be at least 270 bp longer than the $\gamma 2a_s$ mRNA (assuming a 100-bp 3'-untranslated region, as in $\gamma 2a_s$ mRNA). In 2PK-3, as the 3.9-kb species is the only detectable $\gamma 2a$ RNA larger than the $\gamma 2a_s$ mRNA, it must presumably be the $\gamma 2a_m$ mRNA. If this is the case, then it should bear the same V_H sequences as the 1.9-kb $\gamma 2a_s$ mRNA. To test this, we isolated a near full-length cDNA clone of the 2PK-3 $\gamma 2a_s$ mRNA, from which we constructed a V-region probe (probe *a* in Fig. 1A). Figure 2, lane *c* shows that even at high stringency, the V probe hybridized to the 3.9-kb species as strongly compared with the $\gamma 2a_s$ mRNA as did the $\gamma 2a$ probe (Fig. 2, lane *a*). The V probe

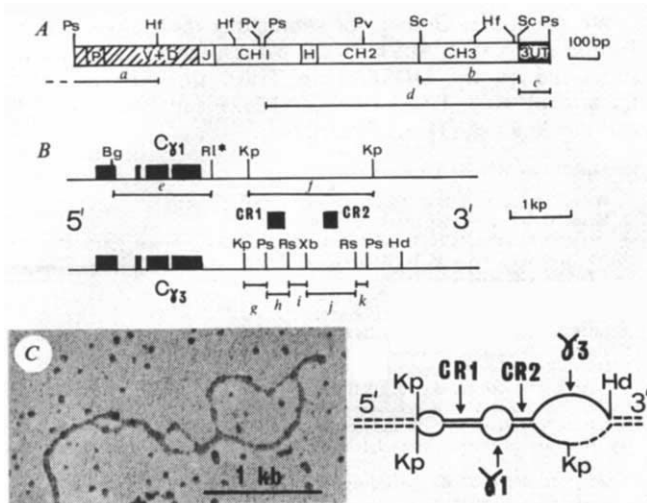


Fig. 1 Probes used to analyse 2PK-3 mRNA. Fragments *a–k* were purified by electrophoresis on 5% polyacrylamide gels. **A** indicates fragments from a cDNA clone (p2PK3 γ 44) constructed from 2PK-3 γ 2a_s mRNA; **B**, fragments from genomic clones containing the C γ 1 and C γ 3 genes²⁰; and **C**, one of the heteroduplex molecules used to localize CR1 and CR2 (see text). Restriction sites are: Ps, *Pst*I; Hf, *Hinf*I; Pv, *Pvu*II; Sc, *Sac*I; Bg, *Bgl*I; RI*, *Eco*RI*; Kp, *Kpn*I; Rs, *Rsa*I; Xb, *Xba*I; Hd, *Hind*III. CR1 and CR2 indicate conserved regions 3' to the genes (see text). To localize CR1 and CR2, fragment *f* from the C γ 1 clone and the corresponding *Kpn*I–*Hind*III fragment from the C γ 3 clone were subcloned in the plasmid pBR322 using mouse DNA 'linkers'. Heteroduplexes were formed between them and spread for electron microscopy at either low (T_m –35°C) or moderate (T_m –25°C) stringency¹⁹. In the schematic diagram in **C**, the dashed lines indicate vector DNA and the solid, insert DNA. γ 3 and γ 1 indicate the subclones of the C γ 3 and C γ 1 clones respectively. CR1 and CR2 represent the regions most frequently hybridizing in the 20 molecules measured.

must have been free of C γ 2a sequences, because it did not hybridize to M173 γ 2a mRNA (lane *d*).

To determine whether, like μ_m mRNA, the putative γ 2a_m mRNA is spliced at the 3' end in a different way to the corresponding secreted-chain mRNA, we hybridized 2PK-3 mRNA with a probe specific for the γ 2a_s 3'-untranslated region (*c* in Fig. 1A). This region commences with a UGA stop codon¹⁶ and must be removed from the γ 2a_m mRNA by splicing if further amino acids are to be encoded at the C-terminus of the γ 2a_m polypeptide. Figure 2, lane *e* shows that the probe hybridized strongly to the γ 2a_s mRNA, but not significantly to the 3.9-kb species (the faint 3.9-kb band is due to slight contamination of the probe with C γ 2a coding-region sequences). Thus the 3.9-kb species is indeed spliced differently at the 3' end. To determine the splice point, we hybridized a DNA fragment spanning the 3' end of the γ 2a_s sequence (*m* in Fig. 3b) to either M173 or 2PK-3 mRNA and determined the lengths of RNA–DNA hybrids using digestion with exonuclease VII¹⁷. Figure 3a shows that mRNA from both M173 and 2PK-3 protected a 600-base fragment corresponding to γ 2a_s sequences (see Fig. 3b). (The doublet at 600 bp is presumably due to the removal from some hybrids of a poly(dT) tract at the 3' terminus of the cloned cDNA which derives from the poly(A) tail of the parental mRNA molecule.) However, in 2PK-3, a distinct γ 2a RNA, presumably the 3.9-kb species, protected a 500-base region terminating near the 3' end of CH3. Hence the novel splice in the 3.9-kb species is located within ~20 nucleotides of the end of CH3, probably, as previously proposed⁵, within the codon (GGT) for the penultimate (glycine) residue of the γ 2a_s polypeptide¹⁶. The surrounding sequences GG/GTAAA closely match the consensus splicing sequence, AG/GTPuAG¹⁸.

To identify genomic sequences complementary to the 3' end of the putative γ 2a_m mRNA, we focused on two regions 3' to the C γ 1 and C γ 3 genes which are as strongly conserved as the gene

coding sequences^{19,20}. These regions, CR1 and CR2, might encode part of the 3' termini of membrane γ 1 and γ 3 mRNAs as the 3' terminus of μ_m mRNA is known to be strongly conserved between humans and mice²¹ and encoded ~2 kb 3' to the rest of the C μ gene⁶. We have now positioned CR1 and CR2 more precisely using heteroduplexes of DNA fragments subcloned in a plasmid vector, as indicated in Fig. 1B, C. CR1 (300 bp long) lies 1.2 kb downstream from the genes, and CR2 (200 bp) 300 bp downstream from CR1. Figure 2, lane *f* shows that a 350-bp fragment 3' to the C γ 3 gene spanning CR1 (*h* in Fig. 1B) hybridized strongly to the 3.9-kb but not the 1.9-kb species. We therefore infer that part of the 3' terminus of the 3.9-kb species is encoded by a region 3' to the C γ 2a gene that is closely homologous to CR1. An 800-bp fragment containing CR2 (*j* in Fig. 1B) hybridized weakly, if at all, to the 3.9-kb species and the less conserved regions represented by fragments *g*, *i* and *k* did not detectably hybridize (data not shown). As expected, a 2.0-kb fragment 3' to the C γ 1 gene which contained CR1 and CR2 (*f* in Fig. 1B) hybridized specifically, albeit weakly, to the 3.9-kb species (lane *g* in Fig. 2). The γ 3 CR1 and γ 1 3' probes also hybridized to minor species of 3.4 and 3.5 kb (arrowed in Fig. 2), but these must be transcripts of the C γ 1 gene because a γ 1 probe (*e* in Fig. 1B) hybridized strongly to both, whereas C γ 2a (lanes *a* and *i*) and C γ 3 (data not shown) probes did not. These species may be analogous to the untranslated transcripts of the C μ gene containing the 3' terminus of μ_m mRNA which have been found in some lymphoid and myeloid cells²².

Taken together, these observations strongly suggest that the 3.9-kb species in 2PK-3 is the membrane γ 2a chain mRNA and is generated by a splice which replaces the 100-bp γ 2a_s 3'-untranslated region with a 2.1-kb sequence encoded 3' to the C γ 2a gene. The 3' position of this splice implies that the additional ~90 amino acids of the membrane γ 2a polypeptide are located at its C-terminus. As CR1 is located 1.2 kb 3' to the C γ 1 and C γ 3 genes, and is strongly conserved between these

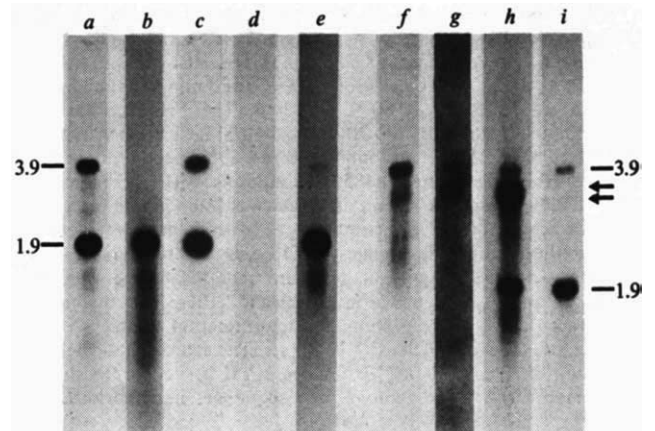


Fig. 2 Hybridization analysis of 2PK-3 mRNA. Polyadenylated RNA was isolated¹⁵ from cultured 2PK-3 cells or from M173 tumours, and 1 μ g per track electrophoresed through a 1.5% agarose gel containing 5 mM methyl mercury(II) hydroxide¹⁵ and then transferred to DBM paper¹⁵. The DBM filters were hybridized overnight at 42°C with 2.5×10^6 c.p.m. ml⁻¹ of ³²P nick-translated probe²³ in 50% formamide, $5 \times$ SSC ($1 \times$ SSC = 0.15 M NaCl, 0.015 M Na₃-citrate) (lanes *a*, *b*, *e*, *h*, *i*); 50% formamide, $2 \times$ SSC (lanes *c*, *d*); or 30% formamide, $5 \times$ SSC (lanes *f*, *g*), plus 0.02% polyvinylpyrrolidone, 0.02% bovine serum albumin, 0.02% Ficoll, 0.1% SDS, 50 mM Na phosphate pH 7.0, 1 mM EDTA, 100 μ g ml⁻¹ denatured salmon sperm DNA, 40 μ g ml⁻¹ poly(C), 20 μ g ml⁻¹ poly(U). After hybridization, the filters were washed with $2 \times$ SSC, 0.1% SDS at 42°C for 3–8 h, dried and autoradiographed. Lanes *a*, *c*, *d*–*i* contain 2PK-3 mRNA and lanes *b* and *d*, M173 mRNA. The probes for lanes *a*–*i* were respectively fragments *b*, *b*, *a*, *c*, *h*, *f*, *e* and *d* in Fig. 1. Lanes *a*–*e* and *f*–*i* were derived from different electrophoresis runs. The sizes of the hybridizing RNA species were determined by reference to mouse 18S and 28S rRNAs which were assumed to be 2.0 kb and 4.7 kb long, respectively²². Arrows indicate the 3.4- and 3.5-kb species.

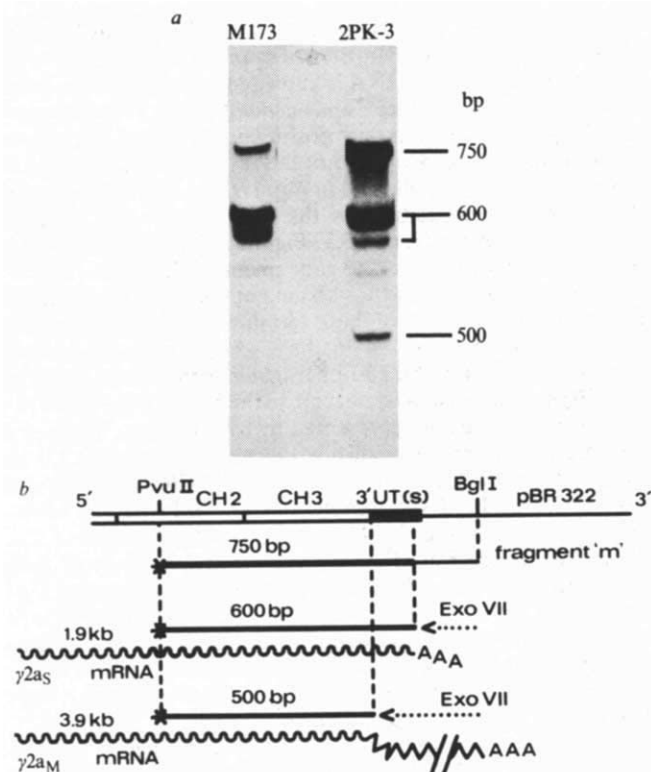


Fig. 3 Localization of the 3' splice site in 2PK-3 $\gamma 2a_m$ mRNA. **a**, Exonuclease VII digests of hybrids between 2PK-3 $\gamma 2a_m$ mRNA and a cloned fragment of M173 $\gamma 2a_s$ cDNA. A *PvuII*-*BglI* fragment spanning the 3' terminus of cloned M173 $\gamma 2a_s$ cDNA²⁴ was labelled with ³²P at the *PvuII* site using the Klenow fragment of DNA polymerase I (ref. 25) and purified by polyacrylamide gel electrophoresis. The DNA was then mixed with total M173 or 2PK-3 poly(A)-containing RNA (20 $\mu\text{g ml}^{-1}$ or 200 $\mu\text{g ml}^{-1}$ respectively) in 10 μl of 60% formamide, 80 mM 1,4-piperazine diethane sulphonic acid pH 7.9, 5 mM Tris-HCl pH 7.9, 0.42 M NaCl, 3 mM EDTA, denatured at 80 °C for 3 min and then hybridized to the RNA at 60 °C for 24 h. The hybridization was terminated by dilution into 200 μl of 30 mM KCl, 10 mM Tris pH 7.5, 10 mM EDTA and unhybridized DNA sequences were removed by digestion with 0.5 U exonuclease VII¹⁷ for 30 min at 45 °C. DNA protected from digestion was fractionated on a 0.2-mm 5% acrylamide gel containing 7 M urea and detected by autoradiography. The undigested DNA at 750 bp derives from reannealed DNA fragments. The faint band located between the 600 bp and 500 bp bands in M173 and 2PK-3 is probably an artefact due to overdigestion. **b**, Determination of the $\gamma 2a_m$ 3' splice site from exonuclease VII digests. The asterisk indicates the position of the ³²P label. Exonuclease VII is specific for single-stranded DNA¹⁷ and removes any sequences not hybridized to mRNA.

two most distantly related of C γ genes, we suggest that it is common to the four C γ genes and encodes the specific C-terminal amino acids of membrane γ chains. Both the presence in the putative $\gamma 2a_m$ mRNA of sequences homologous to CR1 and the length of CR1 (300 bp) are consistent with this hypothesis. Moreover, we have recently learned that several other B-lymphoma lines contain a candidate $\gamma 2a_m$ mRNA of similar size to that in 2PK-3 and that conserved regions resembling CR1 and CR2 occur 3' to the C $\gamma 2a$ and C $\gamma 2b$ genes (R. Wall and T. Honjo, personal communications). The 3.9-kb $\gamma 2a_m$ mRNA is 2.1 kb larger than would be required to encode the 65,000-MW $\gamma 2a_m$ polypeptide. It may therefore have a very large 3'-untranslated region like that of the mRNA for the small-T antigen of SV40 (1.9 kb)¹⁷. Alternatively, it might encode a precursor $\gamma 2a_m$ polypeptide with a long C-terminal sequence. As differential splicing generates mRNAs for membrane and secreted μ ⁵⁻⁷, δ ⁸⁻¹¹ and γ chains (this paper), we suggest that this is the general mechanism by which lymphocytes simultaneously produce membrane and secreted immunoglobulin.

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Purified human growth hormone from *E. coli* is biologically active

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Human growth hormone (HGH), somatotropin, is a protein consisting of 191 amino acids with a molecular weight of ~22,000. It is essential for linear growth and its application in the treatment of hypopituitary dwarfism is well established¹⁻³. Growth hormone may also be effective in the treatment of other disorders including bone fractures, burns and bleeding ulcers⁴. Because growth hormone is species specific, human cadavers have previously been the only source of HGH. Moreover, the standard purification of pituitary-derived HGH does not provide a homogeneous product, but rather a mixture of native HGH and modified forms and fragments of HGH, each of which may have different physiological roles⁵. The expression of HGH in *Escherichia coli* using recombinant DNA technology⁶ provides an inexhaustible source of a single eukaryotic protein. The bacterially synthesized HGH, produced from *E. coli*, is methionyl-HGH (Met-somatotropin), the extra methionine arising from the AUG start codon inserted at the beginning of the gene. This cloned product has now been purified to homogeneity and its biological properties examined. For pituitary and bacterially synthesized HGH, specific activities as determined by radioimmunoassay are comparable.

Bacterially synthesized HGH was purified to homogeneity (by SDS-polyacrylamide electrophoresis gels) starting with *E. coli* extract. Nucleic acids and membrane components were precipitated by polyethyleneimine⁷. The proteins remaining in the polyethyleneimine supernatant were fractionated by

ammonium sulphate precipitation (1.95 M) and the ammonium sulphate pellet was dissolved and chromatographed on diethyl-aminoethyl cellulose (Whatman DE53), CM-cellulose (Whatman CM52). The CM52 fraction containing bacterially synthesized HGH was concentrated by ammonium sulphate precipitation and chromatographed on Sephacryl S-200 (Pharmacia).

The purity of the bacterially synthesized HGH was assessed by SDS-gel electrophoresis, gel permeation chromatography, and isoelectric focusing. The bacterially synthesized HGH appeared homogeneous on SDS-gel electrophoresis⁸ and non-denaturing polyacrylamide gel electrophoresis at pH 10 (ref. 5); the bacterially synthesized HGH and pituitary-synthesized HGH had identical mobilities in both systems. Disulphide dimers are known to have decreased biological activity and cause antibody formation in human patients. However, no dimer was detected in the bacterially synthesized HGH on the pH 10 gels nor was any detected using analytical gel permeation chromatography (LKB Ultragel AcA54). The isoelectric focusing patterns for HGH from the two sources are very similar. Both contain a major and minor species identified by Lewis⁵ as HGH and desamido-HGH, respectively; however, the pituitary-synthesized material contains several minor bands not present in the bacterially synthesized HGH.

Table 1a gives the amino acid composition of bacterially derived HGH. The experimental values obtained agree remarkably well with those obtained from purified pituitary-derived HGH. Automated sequence analysis of bacterially synthesized HGH (Table 1b) shows that the NH₂-terminal residue is methionine and the sequence of the next 17 residues is in exact agreement with that of the published sequence of pituitary-derived HGH⁹. Quantitative recovery of methionine phenylthiohydantion (PTH) on the first cycle of degradation indicates that the preparation contains only free methionine at the NH₂-terminus. The presence of methionine at the NH₂-terminus of bacterially synthesized HGH is consistent with the evidence that formylmethionine initiates protein chains in *E. coli*^{10,11} and that this bacterium contains an enzyme which removes the formyl group¹².

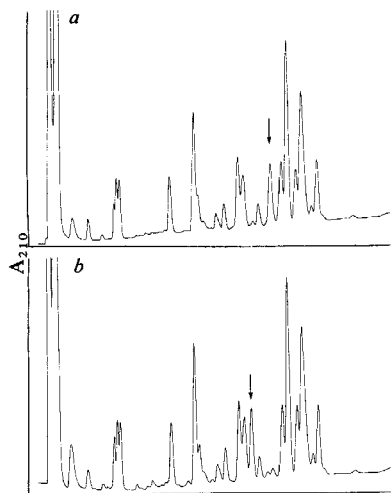


Fig. 1 Tryptic peptide mapping of bacterially synthesized HGH and pituitary-derived HGH. A sample of 1 mg of either bacterially synthesized or pituitary-derived HGH was reduced with β -mercaptoethanol and carboxymethylated with iodoacetic acid according to the procedure of Gurd²⁰. Digestion with tosylphenylalaninechloromethyl ketone treated trypsin (Worthington) was performed by adding 10 μ g in 10 ml of enzyme to 1 mg of reduced carboxymethylated HGH in 1 ml of 1% ammonium bicarbonate. The reaction was allowed to proceed for 12 h at 37 °C. A second aliquot of trypsin was then added, the mixture incubated a further 12 h at 37 °C and the reaction stopped by lyophilization. Peptide mapping was performed as described by Fullmer and Wasserman²¹ using a Spectra Physics SP 8000 HPLC. Samples of 25–50 μ g, at a concentration of 1 μ g μ l⁻¹ in 5% glacial acetic acid, were injected onto a LiChrosorb RP-18 (EM Reagents) column at flow rate of 2 ml min⁻¹. Peptides were eluted with a 50-min linear gradient from 0.1% orthophosphoric acid to acetonitrile, and monitored at 210 nm. Chromatogram *a* is derived from *E. coli*-synthesized HGH, chromatogram *b* from pituitary-synthesized HGH.

Table 1 Amino acid composition and NH₂-terminal sequence of bacterially derived human growth hormone

<i>a, Composition</i>		Experimental values	Theoretical values
Amino acid			
Aspartic acid		20.5	20
Threonine		9.6	10
Serine		17.4	18
Glutamic acid		28.0	27
Proline		8.4	8
Glycine		8.5	8
Alanine		7.0	7
Half-cystine		3.8	4
Valine		6.9	7
Methionine		3.5	3
Isoleucine		8.0	8
Leucine		25.0	26
Tyrosine		8.1	8
Phenylalanine		12.9	13
Tryptophan		ND	1
Lysine		8.8	9
Histidine		3.5	3
Arginine		9.9	11

<i>b, NH₂-terminal sequence</i>			Next largest PTH-amino acid	nmol
Cycle no.	PTH-amino acid	Yield (nmol)		
1	Met	43.4	Ser	19.6
2	Phe	25.6	Ala	2.2
3	Pro	18.4	Phe	4.7
4	Thr*	20.0	Ala	2.0
5	Ile	26.4	Pro	3.4
6	Pro	12.3	Leu	5.6
7	Leu	23.3	Ile	2.2
8	Ser	2.5	Leu	3.1
		(plus PTH-dehydroserine)		
9	Arg	13.9	Leu	4.0
10	Leu	21.7	Arg	2.5
11	Phe	18.2	Leu	3.6
12	Asp	16.8	Leu	4.3
13	Asn	16.3	Asp	5.2
14	Ala	16.4	Leu	2.8
15	Met	18.3	Ala	3.7
16	Leu	18.4	Ala	2.6
17	Arg	10.3	Leu	5.2
18	Ala	13.8	Leu	2.6

In *a*, the experimental values presented are the average value of duplicate analyses performed following 24, 48 and 72 h hydrolysis in the presence of 6M HCl at 110 °C with one crystal of phenol added. The values for serine and threonine were obtained by extrapolation to zero time. The 72-h values for isoleucine and leucine were used. Half-cystine was determined as cysteic acid and methionine was determined as methionine sulphone following performic acid oxidation according to the method of Hirs¹⁹. Tryptophan content was not determined. The theoretical values for the amino acid composition of HGH are based on the data of Li⁹. ND, not determined. In *b*, automated sequence analysis was performed using a Beckman model 890 C sequenator, with a modified program with 0.30 M Quadrol-trifluoroacetic acid, pH 9.0, as coupling buffer. 1 mg of bacterially synthesized HGH was combined with 3 mg Polybrene, dissolved in 0.5 ml distilled water and subjected to 18 cycles of Edman degradation. Amino acid phenylthiohydantoin were identified by HPLC.

* Threonine 2.8 nm plus 17.2 nm dehydrothreonine.

Rather than sequence the entire molecule, we have used tryptic mapping to probe the internal structure of the molecule. Figure 1 shows the HPLC elution profiles of tryptic digests of bacterially synthesized (Fig. 1a) and pituitary-derived (Fig. 1b) HGH. The total number of expected tryptic peptides is 21 (lysine plus arginine = 20), with a free lysine being released due to the Arg-Lys-Asp sequence in the intact molecule. Twenty major peaks are observed with both preparations and the elution profiles are nearly identical, with the exception of one peak which is shifted in the bacterially synthesized HGH tryptic digest. The non-coincident peaks labelled with an arrow in the tryptic digest of bacterially synthesized (Fig. 1a) and pituitary-derived (Fig. 1b) HGH were collected and examined by NH₂-terminal analysis using dansyl chloride. The peptide from the bacterially synthesized HGH contained methionine at the NH₂-terminus whereas the NH₂-terminus of the peptide from pituitary-derived HGH contained phenylalanine. These results indicate that the two peptides are the NH₂-terminal tryptic peptides of each preparation and the difference in elution time

of the bacterially synthesized NH_2 -terminal tryptic peptide is accounted for by the additional methionine.

To determine whether the bacterially synthesized and pituitary-derived HGH preparations were equipotent we determined the weight gain response of hypophysectomized rats to treatments with 3, 10, 25, 50 and 100 μg of the materials given subcutaneously daily for 12 days. The results (Fig. 2) show that essentially identical growth occurred with the bacterial material and with the pituitary-derived material and also with a mixture containing equal weights of the two materials. Regression analyses of the data show that the three lines are not significantly different and that the highest value of $\lambda (=s/b)$ where s is the standard deviation and b the slope of the line) is 0.331, indicating a high accuracy for the method, consistent with previous reports¹³.

To determine whether repeated periods of treatment with natural and bacterially derived HGH were equally effective, we subjected rats which had been treated with HGH, 100 μg per day for 3 days, to a second treatment period of 9 days starting 15 days after completion of the initial treatment. The results (Table 2a) show no significant differences in growth with either pituitary or bacterially synthesized HGH preparations when equal weights of material were administered. The weight gains occasioned by the second treatment period were not significantly different regardless of whether the first or second treatment periods were with the pituitary or bacterially synthesized HGH preparations. These results indicate that the bacterially synthesized HGH does not contain factors limiting growth response to repeated treatment periods, such as increased immunogenicity or adjuvant effects of *E. coli* contaminants. The same conclusion has been drawn from a similar study comparing bacterially synthesized HGH with human pituitary HGH from another source¹⁴.

We were concerned to show that the weight gains of the rats treated with HGH from the two sources correlated with another parameter of body growth. A classical measurement of growth is tibial growth^{15,16}. We therefore determined the overall length of the left and right tibia of each rat receiving HGH, 50 μg per day, or buffer in the dose-response study, using X rays of the animals' hindquarters taken 9 days after the last treatment. Table 2b gives the mean tibia lengths for the treatment groups, the treatments being ranked by increasing mean tibia length. The treatments differed overall at the 1% significance level and

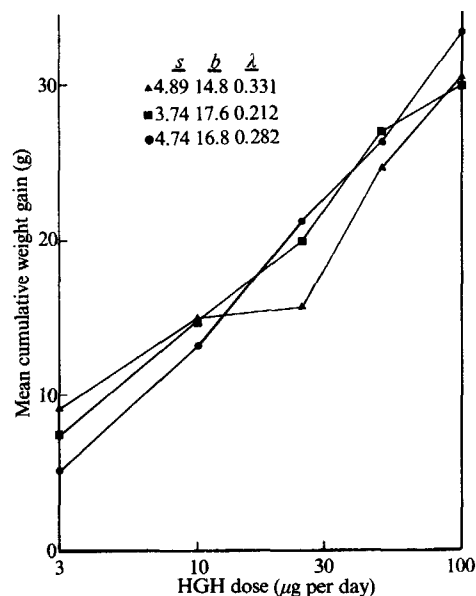


Fig. 2 Dose-response curve for *E. coli*- and pituitary-derived HGH. Mean cumulative weight changes of groups of six hypophysectomized rats after treatment subcutaneously daily for 12 days with various doses of HGH: $\circ$, methionyl HGH (Genentech lot no. C 117-89); Δ , human pituitary HGH (National Pituitary Agency lot no. AFP 4793-B); $\blacksquare$, mixtures of equal weights of the two HGH preparations. The cumulative weight gain of a control group of 12 rats treated with glycine buffer was 2.8 g.

Table 2 Weight gain and tibia growth of rats

a, Weight gain		Weight gain 1st period g $\pm$ s.e.	Weight gain 2nd period g $\pm$ s.e.	Overall weight gain g $\pm$ s.e.
Material administered	2nd treatment period			
Buffer	—	-0.8 $\pm$ 0.8	—	—
GEN HGH	GEN HGH	6.9 $\pm$ 1.1	11.8 $\pm$ 2.6	19.0 $\pm$ 2.4
	KAB HGH	5.7 $\pm$ 1.1	10.9 $\pm$ 2.6	16.9 $\pm$ 2.4
KAB HGH	GEN HGH	6.6 $\pm$ 1.1	17.3 $\pm$ 2.6	22.9 $\pm$ 2.4
	KAB HGH	6.3 $\pm$ 1.1	13.7 $\pm$ 2.6	19.1 $\pm$ 2.4
Treatment/residual <i>F</i>		42.1	1.9	0.9
<i>P</i>		<0.001	NS	NS

b, Tibia growth		<i>n</i>	cm $\pm$ s.e.	<i>t_A</i>	<i>P</i>	<i>t_B</i>	<i>P</i>
Treatment							
Buffer		12	3.055 $\pm$ 0.023	—	—	—	—
NPA HGH		6	3.153 $\pm$ 0.033	2.35	0.05	—	—
NPA + GEN HGH		6	3.176 $\pm$ 0.033	2.95	0.01	0.74	NS
GEN HGH		6	3.188 $\pm$ 0.033	3.16	0.01	—	—
Treatment/residual <i>F</i>			5.10				
<i>P</i>			<0.01				

In a, rats were treated with pituitary HGH (KAB HGH) or bacterially synthesized HGH (GEN HGH) for 3 days followed by 9 days of treatment with these materials starting 15 days after the end of the first treatment period. All treatments were given as 100 μg per rat subcutaneously, 7 hypophysectomized rats per group. NS, not significant. In b, total tibia lengths of hypophysectomized rats treated for 12 days with 50 μg per day pituitary HGH (NPA HGH), bacterially synthesized HGH (GEN HGH) and a mixture of these materials, compared with buffer-treated control rats. Analysis of variance showed that the treatments differed overall at the 1% level. A *t*-test between each HGH treatment with the buffer control gave the *t_A* values and significance levels shown and comparisons of the NPA and GEN HGHs gave the *t_B* value which was not significant. Total tibia lengths were determined from X rays of the hindquarters of anaesthetized rats flattened onto Kodak high-speed BB5 X ray film with a Cronex RB intensifier screen using an exposure of 0.03 s from a Profexray Rocket 400 at a distance of 20 in. using a dose of 100 mA, 40 KVP. Lengths were measured with a machinist micrometer using a hand-held magnifying lens from the proximal end of the tibia (intercondylar eminence) to the bony edge of the talocrural joint in line with the groove of the tibialis posterior tendon.

t-tests between successive groups demonstrated that the buffer control differed significantly from all the HGH-treated groups. However, there were no significant differences between any of the mean tibia lengths of the three HGH-treated groups (see Table 2b). Thus, these tibia growth measurements also demonstrate that the pituitary- and bacterium-derived materials are essentially equipotent. The use of X-rays, as described here, for determination of longitudinal growth of bones seems potentially useful because of the ease of the assay and its nondestructive nature.

The equal biological potency of the various HGH preparations tested proves, for the first time, that the growth promoting property of the pituitary material is attributable solely to HGH and not to synergistic activity with other pituitary hormones or HGH variants known to be present in the natural preparations^{17,18}. The bacterially derived HGH would not be expected to react in radioimmunoassays for prolactin, follicle-stimulating hormone, luteinizing hormone or thyrotropic hormone because only HGH was cloned into the bacteria⁶, and absence of these hormones in the bacterially derived material has been confirmed (A. Parlow, personal communication). Moreover, the diabetogenic effect of human pituitary HGH preparations is apparently associated with contaminating peptides, which should be absent from the bacterially derived HGH. Also, the bacterially derived HGH preparation is free of aggregated forms of HGH and would therefore not be expected to be any more immunogenic than pituitary-derived preparations because neutralizing antibodies in humans to HGH have been associated mainly with aggregated forms¹⁷. No adverse effects were observed in the rats in the dose-response study, even in those given the highest HGH dose. The presence of an *N*-terminal methionine does not seem to affect the biological activity of the bacterially synthesized HGH and this is not really surprising because an HGH variant present in pituitary HGH preparations at about 5% ('fast-HGH') has a blocked *N*-terminal end, probably due to presence of an acetyl group⁵.

We conclude that the described purification procedure for the bacterially derived HGH is such that the material is equipotent with pituitary-derived preparations on a weight basis when administered to rats. The bacterial material is homogeneous by several analytical methods and its growth promoting properties in rats over a range of doses and in multiple treatment regimens indicate that the material should be clinically useful.

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Three-dimensional structure of the enzyme catalase

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Catalase ($\text{H}_2\text{O}_2:\text{H}_2\text{O}_2$ -oxidoreductase, EC 1.11.1.6) is an enzyme that catalyses decomposition of hydrogen peroxide to oxygen and water, and is present in all aerobic cells. All catalases studied so far are tetrameric, each subunit (molecular weight ~60,000) being formed by a single polypeptide chain with haemin as a prosthetic group^{1,2}. Catalase is one of the most efficient enzymes known, resulting in reaction rates approaching the diffusion-controlled limit. Knowledge of the three-dimensional structure of catalase should help to increase our understanding of its mechanism of action, which is at present rather poor. It will also be of interest to compare the structure of catalase with that of other haem proteins, particularly from the point of view of evolutionary relationships. Thus, we have now analysed the three-dimensional structure of catalase from the fungus *Penicillium vitale*. From an electron density map at 3.5 Å resolution it was possible to trace the polypeptide chain. The haem group has been identified, and the active site localized using the specific inhibitor 3-amino-1:2:4-triazole. *P. vitale* catalase subunits have been shown to be composed of three domains, with $\alpha + \beta$, α and α/β type secondary structures respectively. Comparison with the structure of beef liver catalase³ shows that the latter enzyme lacks the C-terminal domain of the *P. vitale* enzyme.

Crystals of *P. vitale* catalase were grown, using the ultracentrifuge method⁴, from a solution containing 17% 2-methyl-2, 4-pentandiol, 0.1 M sodium acetate buffer pH 5.5, and a protein concentration of 0.2 mg ml⁻¹. The crystals were trigonal with $a = 144.4$ Å and $c = 133.8$ Å and belonged to space group P3₂1. The asymmetric unit contained half a molecule. X-ray diffraction data were collected on a four-circle Syntex P2₁

Table 1 Data collection and phase refinement parameters

Compound	Resolution limit (Å)	R_C	R_K	R_S	E/f_H	No. of heavy atom sites	No. of reflections measured
Native	3.5						20,627
$\text{CH}_3\text{HgOCOCH}_3$	6.0	0.13	0.09	0.77	0.95	1	4,541
K_2PtCl_4	6.0	0.12	0.11	0.63	0.63	7	4,380
$\text{Pb}(\text{CH}_3\text{COO})_2$	4.5	0.14	0.11	0.74	0.99	8	9,569
K_2PtCl_6	4.5	0.11	0.07	0.64	0.70	8	9,545
$\text{K}_2\text{Pt}(\text{NO}_2)_4$	3.5	0.12	0.08	0.63	0.68	8	20,361
$\text{K}_3\text{UO}_2\text{F}_5$	3.5	0.14	0.08	0.62	0.62	7	20,711

$$R_C = \frac{\sum |F_P| - |F_{PH}|}{\sum |F_P|}, \quad R_K = \frac{\sum |F_{PH}| - |D_{PH}|}{\sum |F_{PH}|}, \quad R_S = \frac{\sum |F_{PH}| - |D_{PH}|}{\sum |F_{PH}| - |F_P|}$$

where F_P and F_{PH} are the observed structure factors for the crystals of the native protein and of the heavy atom derivative respectively; D_{PH} is the calculated structure factor for the heavy atom derivative; E is the r.m.s. lack of closure error; and f_H is the r.m.s. calculated structure factor for the heavy atoms alone. The overall figures of merit to 3.5 Å resolution before and after two cycles of non-crystallographic symmetry phase refinement were $m_{iso} = 0.63$; $m_1 = 0.85$; $m_2 = 0.89$.

diffractometer; Table 1 lists the heavy atom compounds used for isomorphous replacement. The electron density map at 6 Å resolution revealed that the *P. vitale* catalase molecule has 222 symmetry and that the two subunits in the asymmetric unit are related by a non-crystallographic axis⁵. Thus the molecule of *P. vitale* catalase is found to consist of four identical or nearly identical subunits. The presence of the non-crystallographic symmetry provided a further opportunity of refining the phases of the structure factors. The isomorphous electron density map computed at 3.5 Å resolution has been used to average related

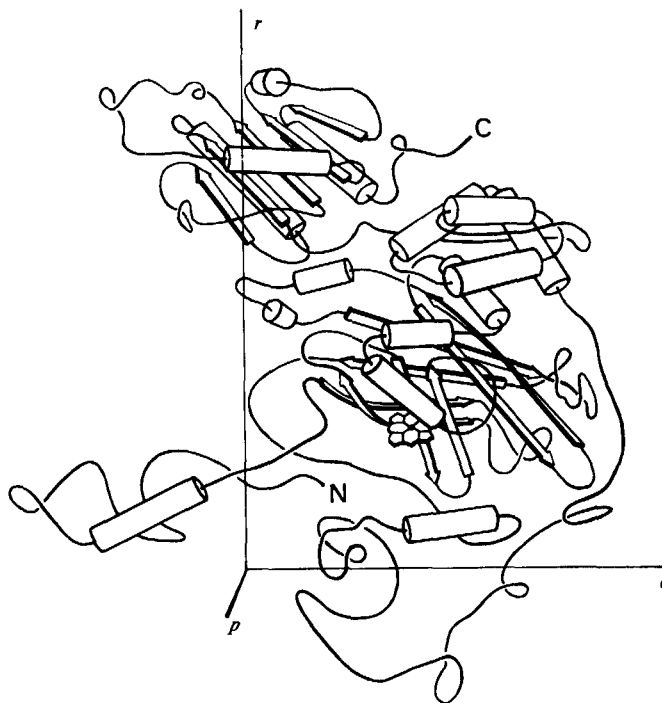


Fig. 1 Schematic drawing of the polypeptide chain conformation for one *P. vitale* catalase subunit. α -helices and β -strands are represented by cylinders and arrows respectively. The N-terminus and C-terminus of the polypeptide chain are marked. p , q and r are 2-fold axes of the molecule. Dimensions of the molecule along these axes are 70, 90 and 120 Å respectively. The relationship between the crystallographic fractional coordinates X , Y , Z and the orthogonal coordinates p , q , r is given by the equation

$$\begin{pmatrix} X \\ Y \\ Z \end{pmatrix} = \begin{pmatrix} 0.0033 & 0.0069 & -0.0022 \\ -0.0033 & 0.0069 & 0.0022 \\ 0.0041 & 0 & 0.0063 \end{pmatrix} \cdot \begin{pmatrix} p \\ q \\ r \end{pmatrix} + \begin{pmatrix} 0.4159 \\ 0.4159 \\ 0 \end{pmatrix}$$

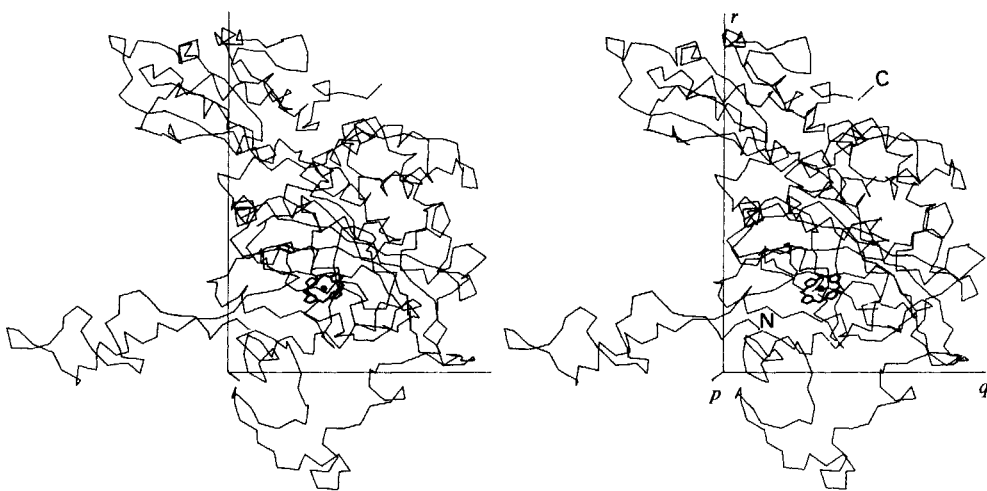


Fig. 2 A stereodrawing of the α -carbon position in a *P. vitale* catalase subunit. The haem group plane orientation in the reference subunit is given by direction cosines of 0.64, -0.34 and 0.71 with respect to the molecular p , q and r axes. The haem Fe coordinates are $p = -3.6 \text{ \AA}$, $q = 16.6 \text{ \AA}$ and $r = 15.3 \text{ \AA}$. The aminotriazole binding site coordinates are $p = -2.9 \text{ \AA}$, $q = 16.6 \text{ \AA}$ and $r = 20.2 \text{ \AA}$.

subunits non-crystallographically and two cycles of phase refinement were applied using the procedure described by Brice⁶ (Table 1). The final electron density map obtained was good enough to trace the polypeptide chain.

The whole chain consists of ~650 peptide groups, ~25% of which exist in helices, with ~18% in the form of extended segments packed in β -sheets. Based on the total number of amino acid residues, the molecular weight of a *P. vitale* catalase was estimated to be 280,000, considerably higher than that of beef liver catalase⁷. Figure 1 shows how the secondary structure elements are arranged along the chain, while Fig. 2 is a stereodrawing showing backbone folding and haem group positions for a catalase subunit.

The polypeptide chain of the catalase subunit can be considered to contain three domains, the largest forming 350 amino acid residues at the N-terminal end, and in accordance with the classification proposed for globular proteins⁸, belonging to the $\alpha + \beta$ type. A remarkable feature of the domain is the eight extended strands which form an anti-parallel β -sheet twisted around to form a closed cylindrical surface. Some strands are separated along the chain by α -helical segments.

An irregular region of about 60 residues connects the N-terminal domain and a smaller domain of 70 residues consisting of four successive α -helices. The helices of the second domain, together with some helices of the N-terminal domain, form one side of the molecular surface.

The third, C-terminal domain contains 150 residues and corresponds in structure to the α/β type. It includes a β -sheet of five parallel strands flanked on each side by two helices. Its topology closely resembles that of flavodoxin⁹, but similar domains occur in several other proteins¹⁰.

Inspection of the regions of contact between the subunits reveals that at the 13th residue the N-terminus of the subunit passes through a loop formed by an irregular segment of the neighbouring subunit.

A preliminary tracing of the polypeptide chain in the subunit of *P. vitale* catalase has been reported, but there was an ambiguity in the haem position¹¹. This ambiguity has been resolved by measuring anomalous X-ray scattering on a crystal of native catalase, assuming iron atoms to be anomalous scatterers. The Bijvoet-difference Fourier map¹² at 8 $\text{\AA}$ resolution revealed two non-crystallographically related haem-binding sites. Each site is situated within the N-terminal domain, at the cylindrical surface formed by the anti-parallel β -sheet (Fig. 2). The distances between iron atoms in the different subunits are 31, 35 and 46 $\text{\AA}$.

Beef liver catalase is specifically inhibited by 3-amino-1:2:4-triazole^{13,14} and we have found that this compound also inhibits *P. vitale* catalase. A set of intensities was obtained from crystals of inhibited catalase at 6 $\text{\AA}$ resolution and the difference Fourier

map calculated relative to the inhibited enzyme showed the inhibitor binding site to be ~5 $\text{\AA}$ from the haem group. The apparent similarity of inhibition by 3-amino-1:2:4-triazole in *P. vitale* and beef liver catalase may imply a similarity in the structure of their active sites. Side groups can be seen, in close proximity to the inhibitor binding site, which occupy chain positions numbered tentatively 57 and 131. Other residues in the haem environment belong to a side of the N-terminal β -sheet and to two α -helices.

A preliminary comparison of the tertiary structures of beef liver and *P. vitale* catalases showed a close similarity in the folding of the polypeptide chain, except that the C-terminal domain of *P. vitale* catalase is absent in beef liver catalase. The position and orientation of the haem groups relative to the molecular axes are very similar in both catalases. There is no doubt that the binding site of 3-amino-1:2:4-triazole is at the active ('distal') side of the haem and that its position in *P. vitale* catalase is near that proposed for beef liver catalase.

A more detailed interpretation of the electron density map of *P. vitale* catalase is difficult because the amino acid sequence of this catalase is unknown. However, some features of evolutionary relationships in the catalase family are already clear. The region of the active site is closely conserved whereas marked changes have occurred in other parts of the molecule. Further insights relating the structure of catalase to its activity will require investigation at higher resolution, as well as studies on the binding sites of hydrogen peroxide and all possible intermediates.

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MATTERS ARISING

Coalitions of male lions:
making the best of a bad job?

BYGOTT *ET AL.*¹ present data from lions (*Panthera leo* L.) showing that coalitions of males are more likely to gain tenure of female prides, retain tenure longer and produce more total offspring than singletons. These results are the first confirmation from field studies that cooperation within groups enhances reproductive success and thus that such cooperation increases the fitness of the individuals involved.

I would like to point out an important feature of Bygott *et al.*'s data which clarifies exactly how an individual male lion gains when joining a reproductive coalition rather than when gaining tenure of a pride himself. Bygott *et al.*'s conclusion that males in coalitions of three or more gain significant reproductive advantages is derived by multiplying the expected reproductive success of males in coalitions of different size by the probability of different-sized coalitions gaining tenure of prides. This calculation results in a single combined measure of reproductive success which increases with increasing group size (see Fig. 1 in ref. 1).

However, there are at least two distinct ways in which individual male lions might gain by joining coalitions: first, males might enjoy a direct benefit, probably due to the more efficient foraging made possible by hunting with other males. Second, the advantage might be only indirect, due to ecological conditions 'forcing' individuals to form coalitions to gain access to a crucial resource^{2,3}.

The data presented by Bygott *et al.* can be readily separated in order to address this issue. From their Fig. 1, the estimated total number of offspring fathered by males during the average tenure of coalitions of different sizes can be obtained (Table 1). These values are lowest for pairs and singletons and highest for coalitions of three to six. Assuming, as do Bygott *et al.*, no skew in male reproductive success, the number of offspring per male is readily derived; this value is greatest for singletons (Table 1). If skew in male

reproductive success occurs, the expected reproductive success for all but the dominant male in groups is likely to be lower than these mean values. As a singleton can expect to produce more offspring during his tenure than the average member of a coalition, individual male lions do not seem to gain directly by joining coalitions. However, data from Table 1 of Bygott *et al.* clearly show that the probability of gaining tenure of a pride is high for coalitions of three or more, but low for singletons and pairs. Clearly, if prides were not limiting, the optimal strategy for males would be to breed as singletons. Thus, the advantage to males in coalitions is indirect, due to the greater probability of obtaining access to a female pride. This conclusion is not altered by the high average relatedness among males in coalitions⁴, as the gain to an individual's inclusive fitness derived through the reproductive success of related males in a coalition will not exceed that gained through independent reproduction of those same males, given that they can obtain access to a pride⁵.

The distinction is important to understanding the evolution of group living. Any male able to gain tenure over and hold a pride by himself should do so; forming coalitions is favourable only when ecological conditions result in severe competition for breeding opportunities among males. In such conditions, males that are unsuccessful at holding a pride themselves are forced to accept the (relatively) inferior alternative of joining a coalition to obtain any breeding opportunity in the population. Thus, forming coalitions may be a conditional strategy evolved among males, many of which would be unsuccessful at gaining tenure of a pride on their own, for 'making the best of a bad job'⁶. This does not imply that coalitions need be composed of males that have themselves tried unsuccessfully to gain tenure of a pride on their own, but only that such a conditional strategy might have evolved on an evolutionary time scale.

Bygott *et al.* ask, "If members of large groups generally enjoy higher reproductive success, why do not all male lions form large coalitions?" This is a serious problem, because less than half (45%) of males in their study were found in large coalitions of three or more. Their explanation of this discrepancy, based on the low reproductive rate of lions and the difficulty of forming coalitions, is unlikely, because if males in large coalitions enjoy direct reproductive advantages there should be strong selection to overcome problems of group formation and to form large coalitions, even if they include unrelated males. However, viewed with the perspective proposed above, this paradox

is resolved: any male that can obtain tenure by himself should do so, and coalitions will only be as large as is necessary to ensure that individual males are likely to be able to gain tenure of a pride.

Maynard Smith and Ridpath⁷ have discussed an analogous situation—that of mate sharing by males in the Tasmanian native hen *Tribonyx mortierii*. Males were found to have higher individual reproductive success as singletons than when sharing a mate with another male, usually a sibling. The authors concluded that the evolution of cooperative polyandry in this species depended on females being a limiting resource. In these conditions, cooperation among males is again parsimoniously explainable as a conditional strategy forced on males failing to obtain a female on their own.

A similar argument attributing group formation to ecological conditions forcing individuals to form coalitions (habitat saturation⁸ or resource localization^{2,5}) rather than to direct reproductive advantages resulting from group living can be applied to many other group-breeding birds, in which *per capita* reproductive success of individuals in groups is lower than, or at best no higher than, that of pairs^{5,9}. Of these group-living vertebrates, however, the most complete evidence on the relative importance of these advantages is that presented by Bygott *et al.* on lions. Those data strongly suggest that male coalitions cannot be explained by any intrinsic advantage to living and cooperating in groups, but rather as making the best of ecological circumstances which force some individuals to form coalitions to gain any sort of breeding opportunity.

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Table 1 Total and per male reproductive success of lions during the average tenure of coalitions

No. of males in group	Total reproductive success	Reproductive success per male
1	8	8
2	4	2
3	14	4.7
4	10	2.5
6	30	5

Data from ref. 1, Fig. 1.

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BERTRAM ET AL. REPLY—Koenig is spelling out as space prevented us¹ originally from doing that there are a variety of possible ways in which male lions (*Panthera leo*) may benefit from cooperating in large long-term breeding coalitions. We concentrated on large (≥ 3) as opposed to small ($=2$) coalitions and showed an absolute and *per capita* increase in reproductive success with increasing coalition size; this increase was obtained partly through an increase in the coalition's chances of gaining tenure of a pride and partly through an increase in the time for which they could retain it. Both gaining and retaining tenure are presumably assisted by a combination of Koenig's 'direct' and 'indirect' advantages, which are difficult to distinguish. For example, it is likely that the enhanced hunting success of larger groups², and their advantage in competitions with rival scavengers³ such as hyaenas (*Crocuta crocuta*) and other lions, will improve the general condition of the coalition members and therefore help them to fight rival coalitions before and during pride tenure. 'Intrinsic' as opposed to ecologically imposed advantages are surely inseparable.

Singletons are an interesting case. We¹ presented data on only two singleton males (compared with 70 males in coalitions) because single males rarely hold prides. Those two who did manage to had reproductive success not significantly different from that of the far more numerous males in coalitions. Thus the data do not justify considering the formation of coalitions in lions to be 'making the best of a bad job'. It seems more reasonable to consider them, as with many other adaptations, as a way of making a good job better.

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Senegal River runoff

FAURE AND GAC¹ have used an unweighted 7-yr running mean of annual Senegal River runoff to predict future rainfall trends in the Sahel. They forecast a return to wetter conditions by 1985. Several questions arise from their analysis.

The first is whether the Senegal River runoff reflects Sahelian rainfall. The river rises in the more humid mountainous region to the south of the Sahel. We have correlated the published runoff figures in

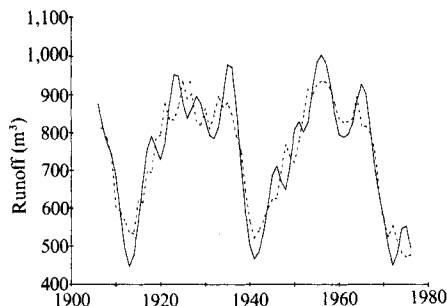


Fig. 1 Filtered Senegal River runoff flow. —, 7-yr binomial filter; ---, 7-yr moving average.

the paper with normalized annual rainfall departures for the Sahel (available up to 1975) (ref. 2 and S. E. Nicholson, personal communication). The correlation coefficient is 0.76 which is significant at the 0.1% level. Therefore, the runoff data are a reasonable proxy for Sahelian rainfall.

The second problem concerns the use of unweighted running means. These are notorious for introducing spurious cyclicity into the data, and for shifting the position of peaks and troughs³. We performed a spectral analysis on the runoff data (which are approximately normally distributed) and found strong cycles at around 30 and 2.29 yr. This shows reasonable agreement with Faure and Gac. We then introduced a 7-yr binomial filter to the raw runoff data, which produced the curve shown in Fig. 1. It can be seen that the 1975 trough has now moved back to 1972, and the smooth upward trend of Faure and Gac has been lost. On this basis, the return to wetter conditions by 1985 can no longer be assumed. It is by no means certain that the 30-yr cycle will be repeated for a third time. This points clearly to the dangers of a prediction based on a record length only double that of the wavelength of the cycle. The statistical problem is further reinforced by Nicholson's evidence that synoptic conditions in the two recent drought phases were quite different (ref. 2 and S. E. Nicholson, personal communication).

One final problem is the use by Faure and Gac of gradient lines *a* and *b* (in their Fig. 4) as estimation limits of the slope of return to wetter conditions. Because they are based on a sample of two, they give an unjustified air of accuracy to the results.

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FAURE AND GAC REPLY—We are grateful for the statistical demonstration of Palutikof *et al.* that the Senegal River discharge data are a good proxy for

Sahelian rainfall, although this has long been known by inhabitants, nomads and those who have studied the Sahel. The spectral analysis and binomial filtering of the discharge data by Palutikof *et al.* are in good agreement with our Fig. 4¹ and illustrate well the general tempo of Sahelian drought in this century.

However, the conclusion of a return to wetter conditions after a drought, and the approximately 30-yr cycle, are not based solely on this century's discharge data for the Senegal River. Our conclusion is rather the result of a convergent series of arguments from various disciplines and data at several time scales, which yield evidence of a dramatic fluctuation in rainfall over recent centuries with period of several decades between wet and dry peaks (Fig. A). It is these data which we

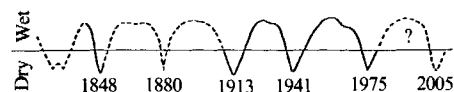


Fig. A General trend of annual rainfall in Sahel from historical information (modified from ref. 3).

primarily use to establish the existence of the rough time scale of fluctuation, and which would be enough to inform us that severe Sahelian drought can be expected at time intervals of several decades. The river discharge data merely provide more accurate time series data. We use this to argue that if the inferred fluctuations of the past few centuries were similar to those observed during this century, then the next severe droughts could be expected in about 2005, with an intervening wet period around 1992.

Geological, prehistoric and historical data in this region² show a variability of climate in which the inferred rates of change are a function of the time scale of study.

Drought is not a simple mathematical, statistical or philosophical phenomenon. It is a natural phenomenon, and must be studied as such by a multidisciplinary approach.

We thank G. S. Boulton for clarifying and rewriting our reply.

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